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A Quarterly Review of Archaeology



Edited by O. G. S. Crawford, F.S.A.

and

Roland Austin, F.S.A

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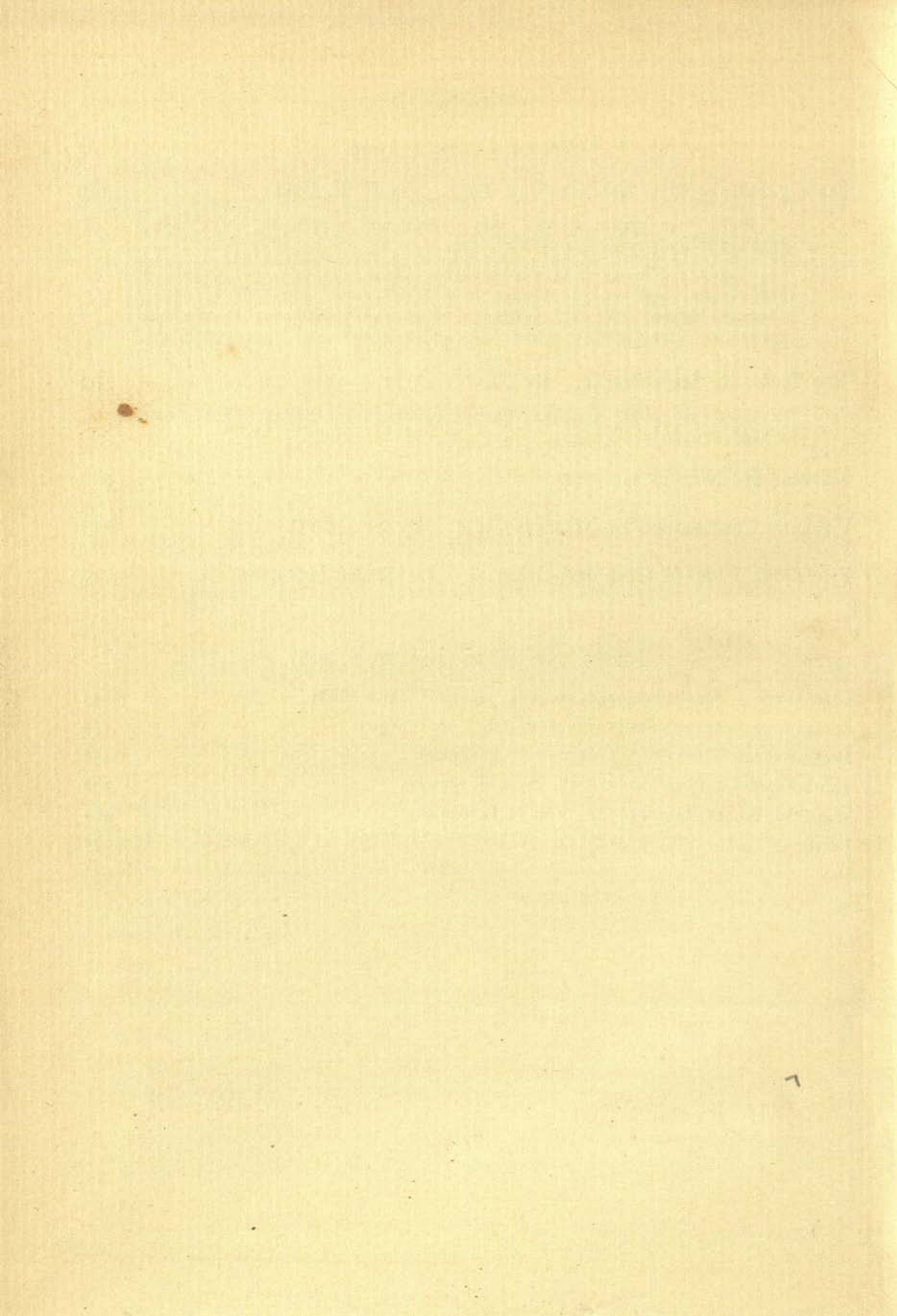
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MARCH 1939

Editorial Notes

NOT long ago we suggested that archaeologists (and others too) could learn much from the study of air-photographs of primitive homesteads and their adjacent cultivations. Such views enable one to visualize these settlements far more clearly and completely than one could from a ground view or even from a plan. Supplemented of course by a more leisurely investigation on the spot, they provide comparative material that assists the excavator in his imaginative reconstruction of prehistoric sites. They also have an intrinsic value and interest of their own for the anthropologist who is concerned with primitive economy.



It was therefore not altogether a surprise to find a number of striking illustrations of these remarks in the October number of the GEOGRAPHICAL REVIEW (published by the American Geographical Society, Broadway at 156th Street, New York). These were taken by Richard and Mary Upjohn Light during a cruise from Capetown to Cairo. They are not confined to primitive native settlements, but show also the up to date farms and plantations of white settlers. The authors took the trouble to qualify themselves beforehand in photography as well as in aeronautics; and being fortunately independent of commercial control, were able to pursue a leisurely course and take an intelligent and lively interest in the marvellous country they traversed. Their

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cruise of $2\frac{1}{2}$ months contrasts vividly with stunt-flights undertaken to advertise some kind of oil or engine ; for it achieved results of real social and scientific value.

The result is a series of vivid pictures of life in Africa such as we have never before seen. It has been left to citizens of the United States of America to do this traversing the whole African continent, and flying throughout over the territory of the British Commonwealth and Egypt. The authors of the flight have brought back with them about a thousand air-photographs altogether, of which about 650 were taken in Africa, and 1800 Leica photographs taken on the ground. It is good news that the American Geographical Society, where a set of them has been deposited, 'has under consideration the publication of a large selection . . . in book form'. May we here express the hope that this project will be carried out, and that the plates will be as large as possible, so that all-important details (of huts, cultivations, roads and such like) may be plainly visible ? One cannot use a magnifier with effect on a half-tone plate.

Is it too much to hope that some day the British Commonwealth will do something for itself now that America has shown the way ? To expect anything from the shopkeepers' island itself is perhaps too much ; but there should be intelligent people in Africa who appreciate the interest and beauty of their own continent. These native villages represent a stage in the development of human society that, as the photographs show, is rapidly giving place to another. The time to record them is now, before they have passed away. We commend these remarks to our numerous readers in South Africa, in the hope that they may take up the matter.

The present state of affairs is not one with which we in this country can be content. Directly or indirectly we control, or are politically and commercially interested in, a very large portion of the earth's surface ; we have the means and opportunity of forming a magnificent collection of air-views of every kind of land, from the Arctic Regions

EDITORIAL NOTES

to the Tropics. Our aeroplanes fly over the whole of Africa and large parts of Asia and Australia. Many air-photographs are being taken all the time, either officially by the Royal Air Force (for training or survey purposes), or by private individuals. Yet there is no central store where negatives can be deposited when they have served their purpose. Many of them are destroyed when their immediate purpose is fulfilled, though they are original documents of great scientific value. They are of educational value also to students of geography, geology, forestry and economics, and are in demand by such (and by others) to illustrate books and articles. Compared with the original cost of taking them, the cost of storage and handling such material is trifling; but it is nobody's business to organize a system of preservation and of making them accessible to the public (who have paid for them).

There is another optimistic forecast that has, it seems, been fulfilled, though again we do not suggest any relation of cause and effect. Ebba de Geer has succeeded in equating the growth-rings of trees from the ancient mound called Raknehaugen, in the Romerike district of Norway, with those of a giant Californian Sequoia on the one hand and with clay-varves from Angermanland in North Sweden on the other.* The mound is a well-known landmark and local antiquity, and is 60 feet high. A shaft sunk in it in 1868-9 revealed a complex timber structure, and at the base a layer of birch and cloven pine-logs. No human remains were found; only the remains of a horse, badly preserved. Some of the pieces of timber were kept locally as souvenirs, and thus became available for the present investigation. It is concluded that the trees were cut, and the mound therefore built, in A.D. 931.

It would be rash to accept the full implications of this most important research until it has, quite properly, been fully considered and perhaps subjected to criticism, and until it is supported, as we hope it will be, by confirmatory evidence from other work of the same kind. One hoped that it might be possible to apply the method of dendrochronology to European material, but it seemed that differences of

* Account by Ebba Hult de Geer in the *Universitetets Oldsaksamlings Arbok*, 1937, pp. 27-54. (Brøgger, Oslo, 1938).

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climate might prevent this. Apparently, however the variations of growth may be associated with solar radiation, and thus be independent of climate. If that be so, and the method can be used here, we shall have a magnificent opportunity of dating not only our own early historic monuments but also prehistoric ones. Any site, such as a lake-dwelling, a tree-trunk burial or a dug-out canoe that can provide a section, will automatically date itself, when subjected to treatment. This, if it becomes possible, will convert archaeology into an exact science where matters of chronology are concerned, and the assignment of date is of course the first object of every archaeologist. One looks naturally to East Anglia and its cultural centre in the hope that yet another Scandinavian technique may be learnt and applied in that receptive quarter.

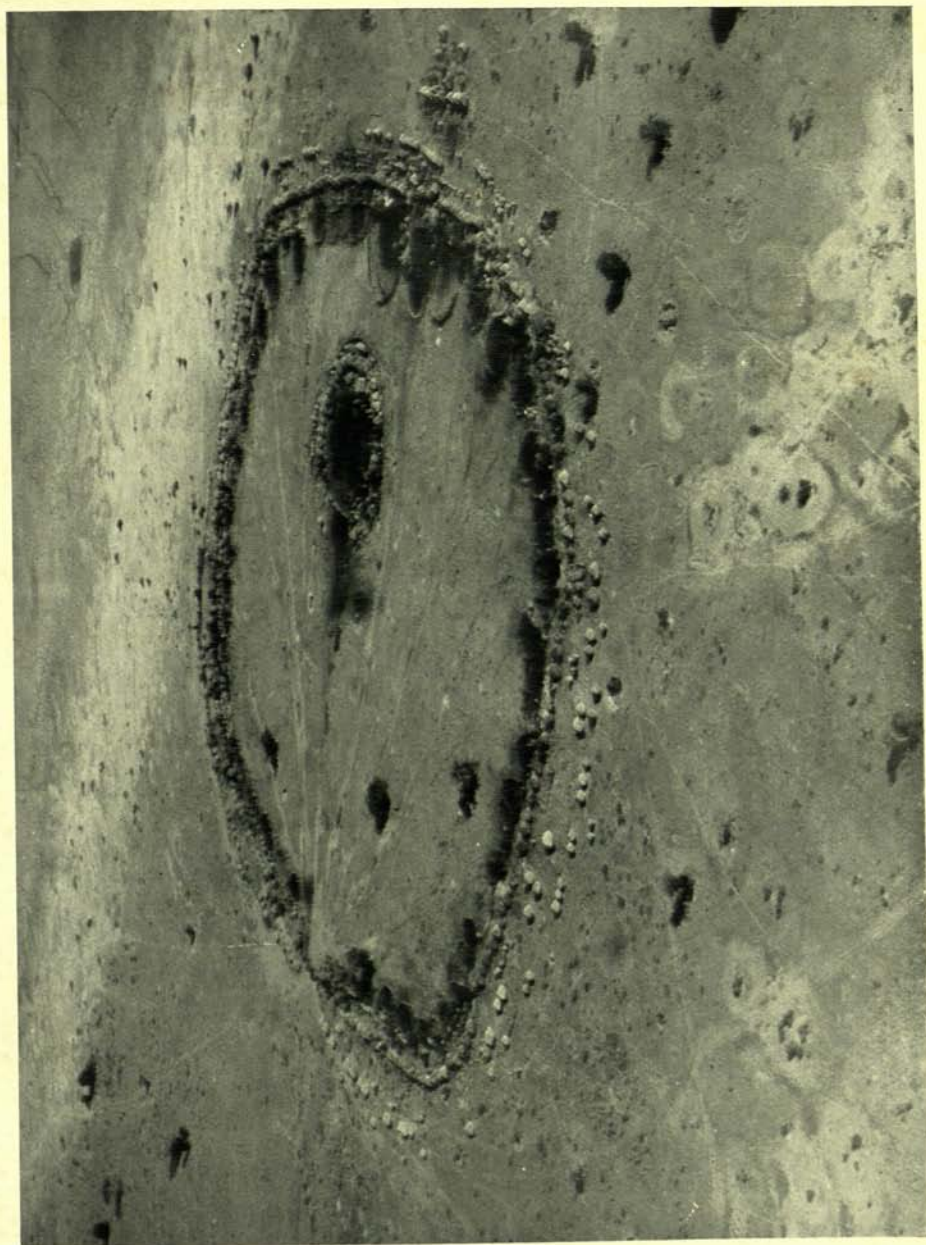


Subscriptions for 1939

The present number begins a new year—subscriptions for 1939 are therefore due. We are grateful to those who have already sent their cheques, and we ask those who have not done so (always excepting those who pay through their bank or bookseller) to remember that payment without formal request is doubly appreciated, for it saves a deal of correspondence.

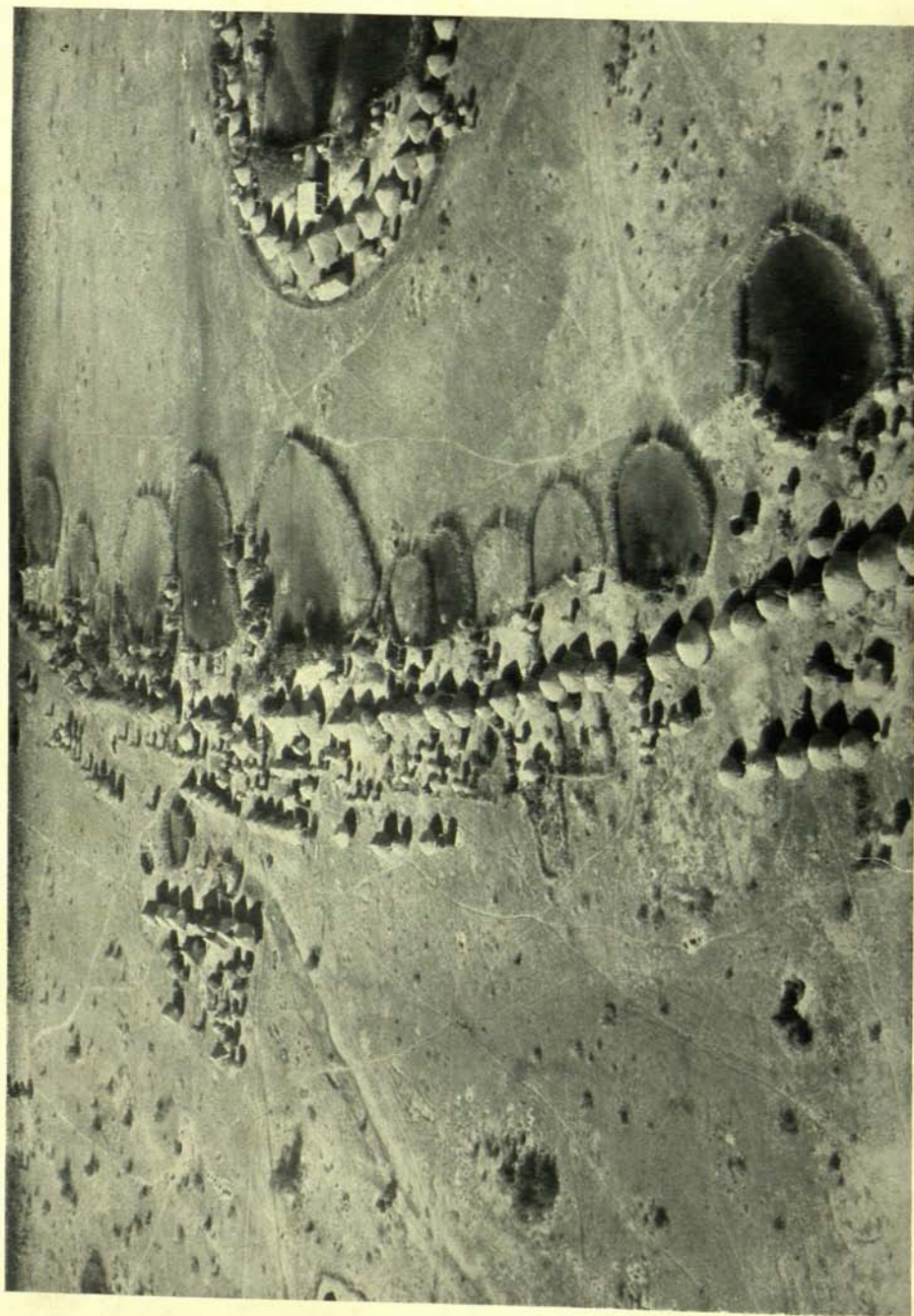
A form and envelope were inserted in the December number.

PLATE I



THE VILLAGE OF MUKOBELA, NORTH RHODESIA, WHICH SHELTERS ABOUT 2000 OF THE BA ILA TRIBE.
THERE ARE NEARLY 1000 HUTS AND A CORRAL FOR 5000 HEAD OF CATTLE
Courtesy of the American Geographical Society and Richard and Mary Upjohn Light

PLATE II



A MORE DETAILED VIEW OF PART OF THE BA ILA VILLAGE AT MUKOBELA, NORTH RHODESIA, SHOWING THE STRUCTURES WHICH FORM THE VILLAGE CIRCLE. THE OTHER STRUCTURES ARE KITCHENS, DWELLINGS, THE STOCKADE, AND CATTLE PENS

Courtesy of the American Geographical Society and Richard and Mary Uppjohn Light

India and the West before Darius

by V. GORDON CHILDE

BEFORE 1918 it was believed that India's contact with the western world began about 3000 years ago with the intrusion into a barbaric peninsula of the half-legendary Vedic Aryans. It was only with the Empire of Darius that India began to figure substantially in the historical records accessible to us. In 1938 we know in the Indus valley a fully-literate and very advanced urban civilization flourishing fully fifteen centuries before the supposed date of the Aryan invasion. We know too that the ancient Indus cities were in regular and intimate contact with the Sumerian cities of Iraq; a stream of Indian manufactures flowed into Mesopotamia, Indian ideas could travel by the same channels. I need not remind you how deeply modern Western civilization is indebted to the ancient Sumerian, how many of the fundamental inventions that make civilization possible are traceable to the Tigris-Euphrates region, how intimately, through the Hebrews and the Greeks, Mesopotamian traditions have been injected into the very core of our spiritual culture. There lie the roots of our mathematics and astronomy, of the myths that, through the Bible, colour our outlook from early childhood. But we can now infer that India too contributed to the formation of the cultural tradition we thus inherit.

This significance of the new world opened to history by archaeology has hardly been adequately appreciated, and a brief account of the stages of discovery and a description of the civilization¹ revealed is still a necessary prelude to a discussion of its significance to ourselves. Bricks from an ancient ruin at Harappa on the Sutlej were used last century as ballast for the Karachi-Lahore railway, and in quarrying the ruins some unfamiliar seals turned up. One was brought to the British Museum, there to lie in lonely isolation for half a century. In 1921 Rai Bahadur Daya Ram Sahni began systematic excavations in the ruins on behalf of the Archaeological Survey of India and in 1922 Mr Rakal Das Banerji discovered below a Buddhist *stupa* at

¹ This article contains the substance of a lecture delivered before the Warburg Institute on 10 October 1938.

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Mohenjo-daro² in Sindh earlier ruins that yielded seals and other relics like those of Harappa. Both sites were subsequently further explored under the direction of Sir John Marshall, revealing a civilization quite different from and older than anything hitherto known to Indian archaeology. But it was not till views of the sites and relics were published in the *Illustrated London News* that the age and affinities of the new civilization could be even estimated. Then the veteran Assyriologist, A. H. Sayce, recognized startling similarities in the fresh Indian material to the Sumerian. Further discoveries not only in India but also in Iraq have triumphantly confirmed his genial diagnosis. In particular the imported Indian commodities found in well-dated strata in Sumer and Akkad leave no doubt that the civilization first identified at Harappa was flourishing before 2500 B.C.

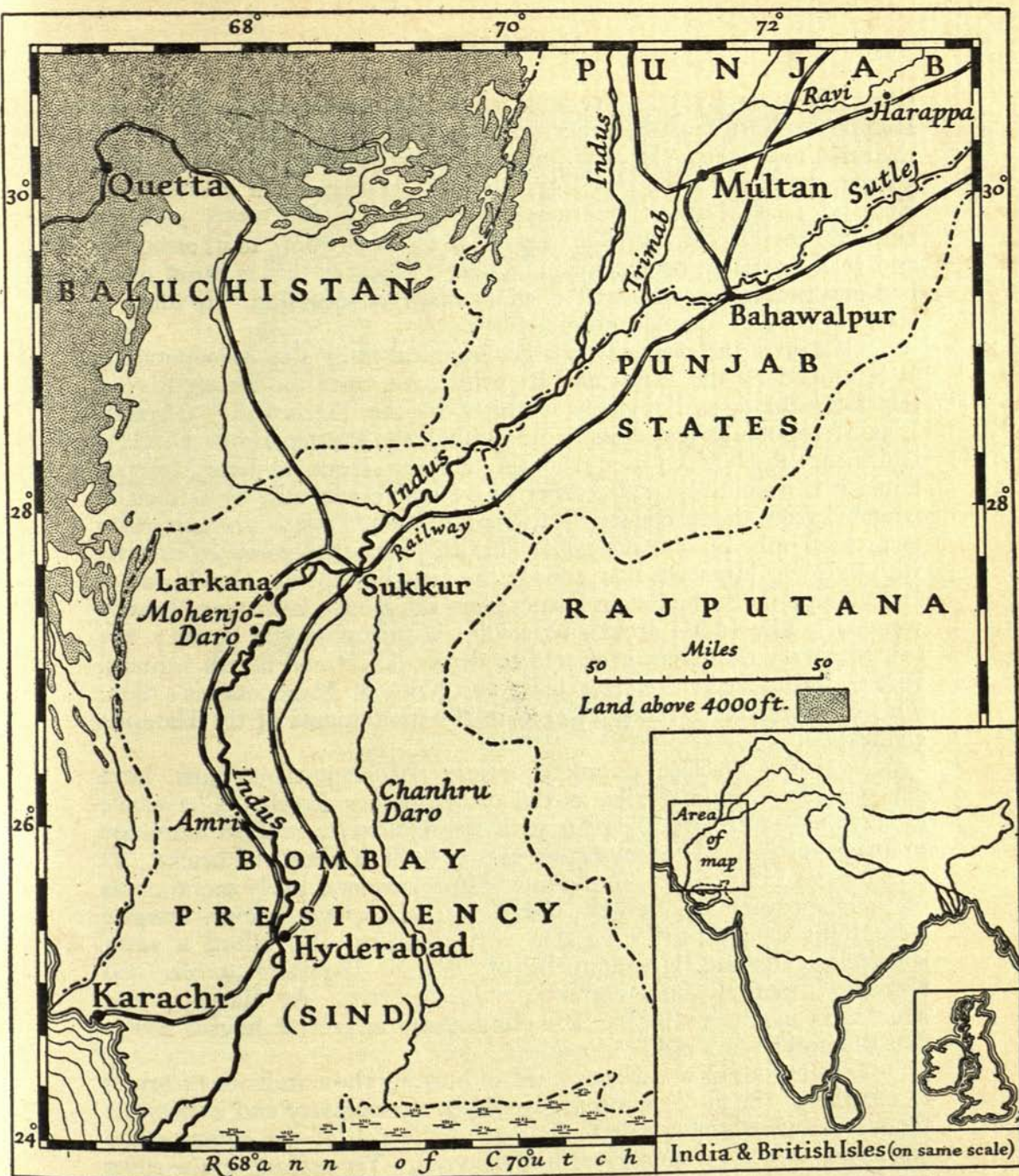
How much earlier it may have begun is uncertain. The civilization revealed at Harappa, Mohenjo-daro, Chanhudaro and other sites is now seen to illustrate only one phase in a long cultural evolution, throughout which a continuity of specifically Indian traditions is evident. The term Indus Civilization must, as Dr Mackay first insisted, be reserved for the whole cycle; the best known phase of it can conveniently be designated the Harappa period (culture, or civilization) after the site where it was first revealed. The Harappa civilization was quite mature and certainly endured for a long time. But only at one site, Amri,³ have remains of a different and earlier culture been disclosed beneath the ruins of Harappa age. At other Harappa sites the groundwater has hitherto prevented deep diggings such as should reveal ruins of the Amri phase. Later phases in the development of the Indus cycle have been defined by Dr Mackay's excavations at Chanhudaro⁴ and will be briefly mentioned later. At the moment I must confine myself to the Harappa period.

Cities of this period have been found from the Indus delta to the middle Punjab. In all that have been explored an astonishing cultural homogeneity reigns: architecture, metalwork, pottery, jewellery, religious symbolism and artistic styles are substantially identical at all

² The only general account of the Indus Civilization is the book thus entitled by Dr E. Mackay, published in London in 1935. An authoritative account of the excavations at Mohenjo-daro down to 1921 with a general survey of the results is contained in Marshall, Mackay, and others, *Mohenjo-daro and the Indus Civilization*, Probsthain.

³ *Memoir Archaeological Survey of India*, 48 (1934), 26 f.

⁴ Provisional reports in *Illustrated London News* 14, 21, November 1936 and *Journal Royal Society of Arts*, 1937, LXXXV, 527 ff.



MAP ILLUSTRATING THE AREA OF HARAPPA CULTURE (see p. 6)

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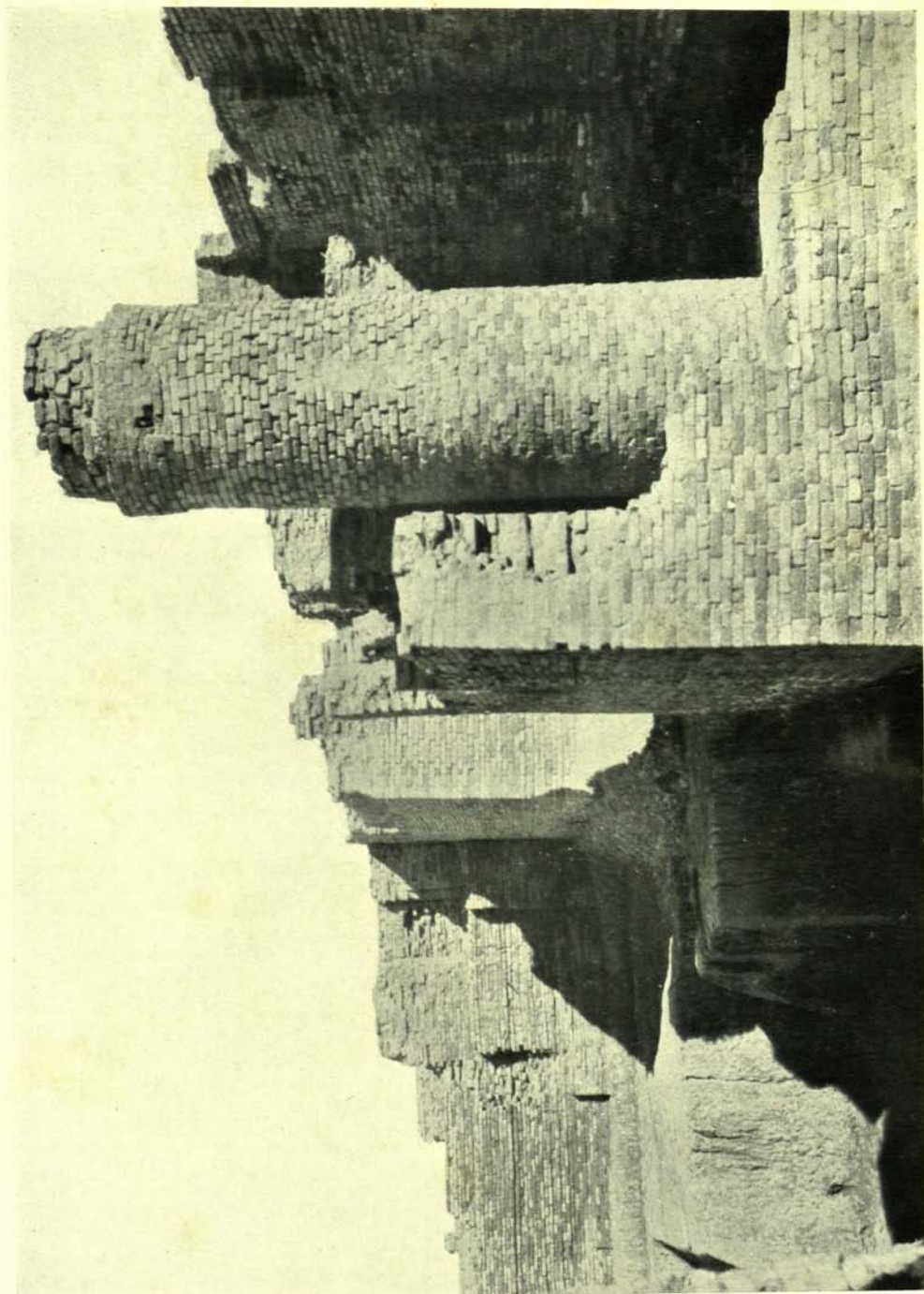
sites. Judging by stray ceramic finds the area embraced by the Harappa culture must have been even larger. It is in any case greater than that over which classical Sumerian civilization extended uniformly, greater than Old Kingdom Egypt, and of course much greater than England (see MAP). The mere dimensions of the newly revealed cultural province constitute an impressive testimony both to its antiquity and to its potency as a civilizing force. Whether this cultural unity had any political counterpart is an unanswered question. It must at least correspond to an economic unity.

Of course the province is a geographical unity like Mesopotamia. It is unified by the Indus and its tributaries, once the Seven Rivers, Sapta Sindhava, as Babylonia is united by the Tigris and Euphrates. Like Mesopotamia it gets its name through the Persian Hapta Hindava and thence the Greek India, from the unifying streams. Like Mesopotamia it is an alluvial plain, desert, save where naturally or artificially irrigated from the streams. But it is not a sandy waste whose horizon is broken only by the mirage. The salt-encrusted earth is covered with ugly scrubby trees that impede the view but not the blowing dust. In Harappa times the Seven Rivers plain must have been more heavily wooded. The Indus artist's repertoire is full of jungle beasts; the contemporary Sumerian preferred to depict desert and marsh animals. In the Indus basin the tiger takes the place of Mesopotamia's lion. This environment is reflected even in the monuments of the Harappa civilization.

Its ruins disclose populous cities: Mohenjo-daro must have covered over a square mile, as did contemporary Erech. The whole is laid out on an orderly plan with regular streets and an elaborate drainage system. The houses are mostly built of kiln-fired bricks. As a result the Indus ruins are the most impressive and lively monuments of their age that can be seen today. In Mesopotamia even temples were built of mud-brick so that their remains now afford a sorry spectacle. In Old Kingdom Egypt, despite imposing tombs and temples, practically no domestic ruins survive. At Harappa and Mohenjo-daro the visitor can still climb stairs in private houses nearly five thousand years old.

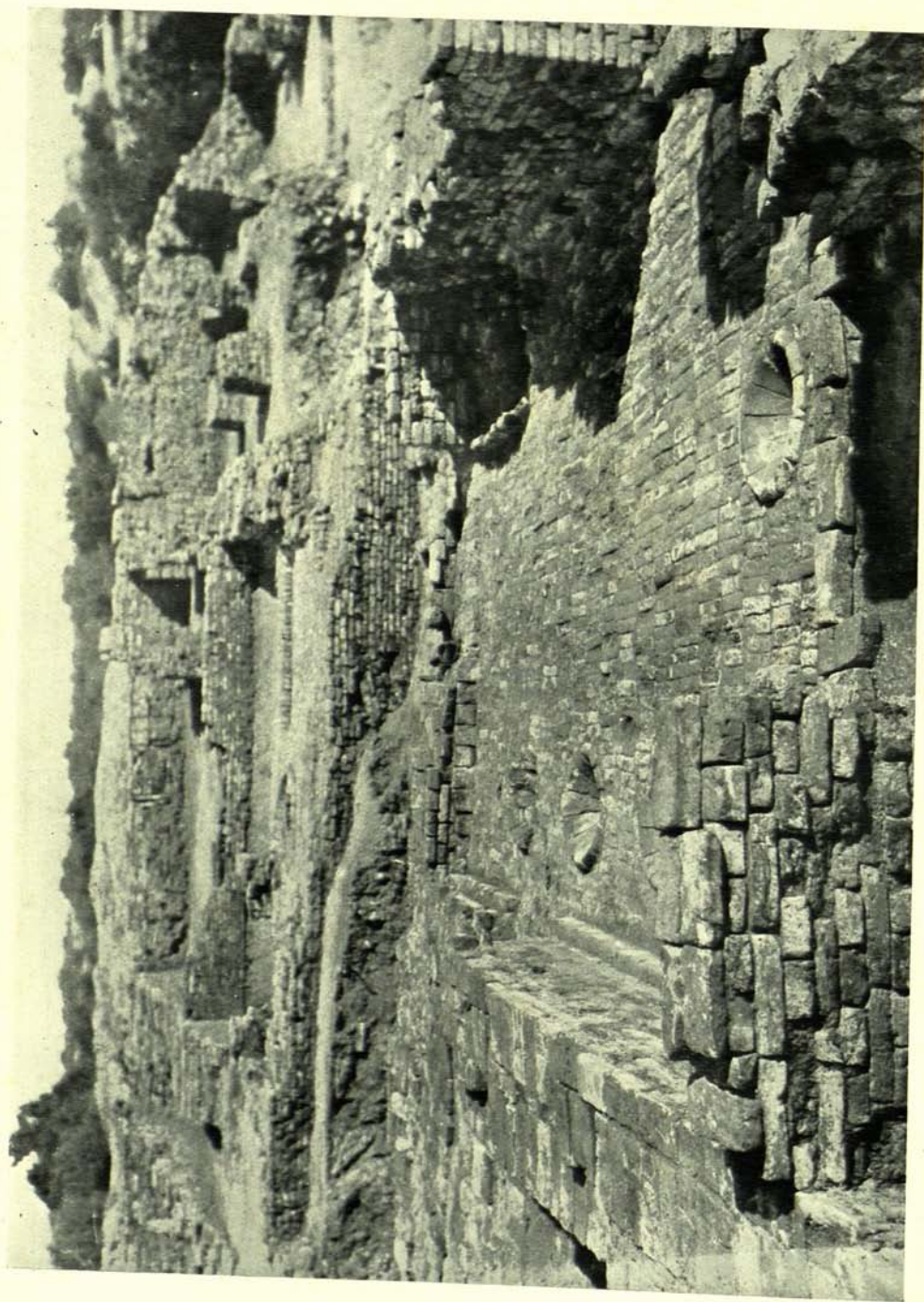
The amount of wood consumed in burning these millions of bricks is appalling. Such consumption would be unnecessary and impossible today. It affords eloquent testimony to the size of the communities concerned and the resources at their disposal. Yet nothing in the ruins indicates the concentration of this wealth. In Mesopotamia the most

PLATE I



WELL LEFT EXPOSED IN DEEP DIGGINGS AT MOHENJO-DARO (see p. 9)
Ph. V. Gordon Childe

PLATE II



WATER-STALL AT MOHENJO-DARO (see p. 11)
Ph. V. Gordon Childe

PLATE III

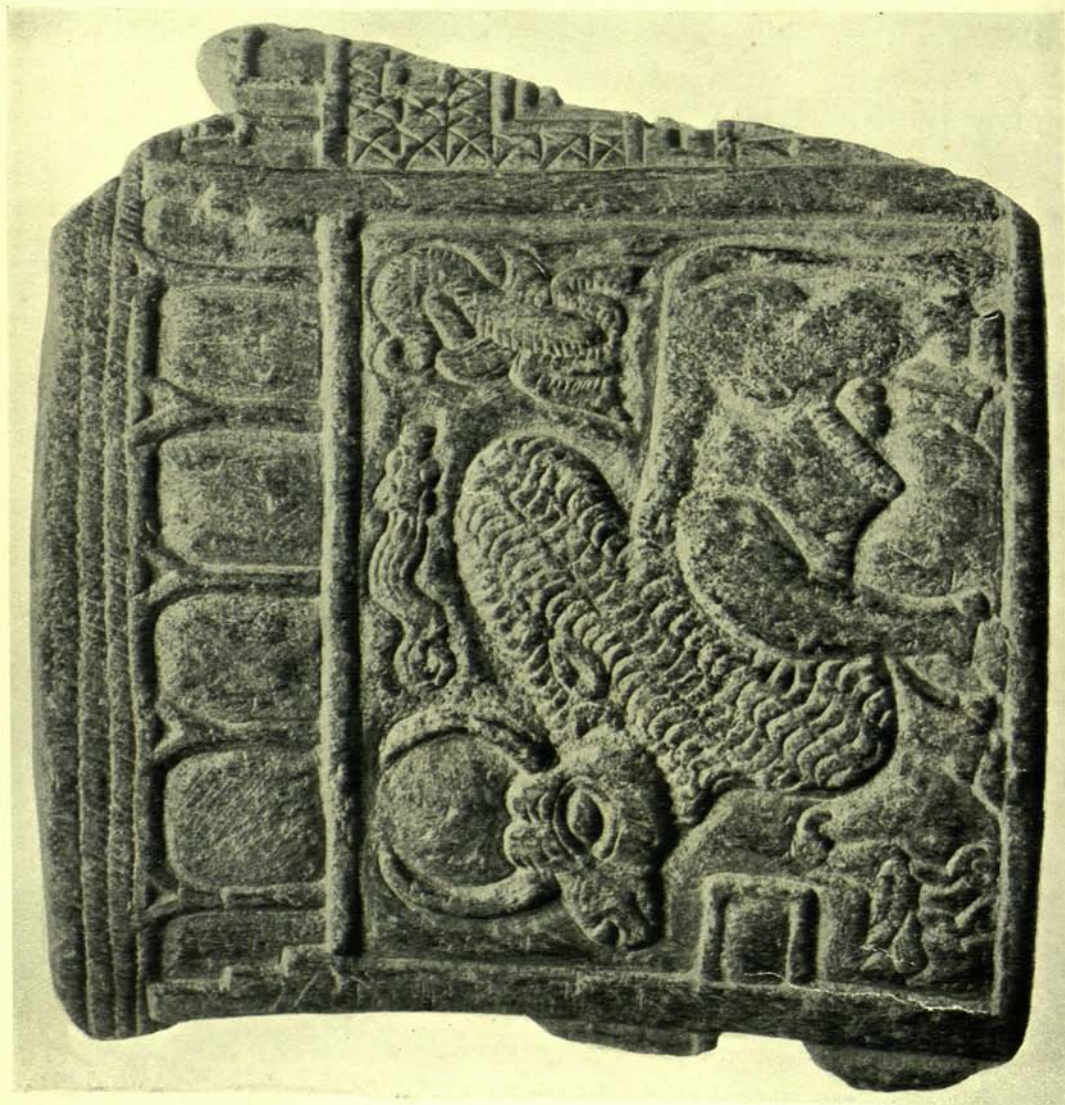


CYLINDER SEAL FROM ESHNUNNA (see p. 12)
By courtesy of the Oriental Institute, University of Chicago

PLATE IV



(2) BRONZE STATUETTE OF DANCING GIRL
(see p. 10)
Archaeological Survey of India



(1) FRAGMENT OF STONE VASE FROM TEL AGRAB (see p. 12)
By courtesy of the Oriental Institute, University of Chicago

INDIA AND THE WEST BEFORE DARIUS

conspicuous ruins are those of temples ; they reveal as clearly as the written texts the role of the city deity as the great capitalist. The pyramids and *mastabas* of Egypt tell the same tale of Pharaoh and his feudal nobles. No such monumental buildings catch the eye in Indus cities. At Mohenjo-daro the most imposing single complex is a great bath. We can distinguish between the slummy dwellings of artizans and the more spacious houses of prosperous merchants—a proletariat and a bourgeoisie perhaps, but not a narrow oligarchy of high finance nor landlords, monopolists by birth.

Mohenjo-daro and Chanhu-daro were more than once overwhelmed by floods and more than once rebuilt. At the former site the new buildings followed closely the plans of the old : the established lines of streets were scrupulously conserved, attesting a persistent tradition of municipal authority. And in both towns the continuity of culture is complete. Old wells were carried on by fresh tiers of brickwork to each higher level, and have been left by the excavators standing like huge chimneys as indicators of the successive floors (PLATE I).

Bricks, houses and streets provide only the bare skeleton of city life ; they must be clothed with an economy and suffused with a life of science and art. An aggregation of peasants does not constitute a city ; such must be inhabited also by artizans, traders and functionaries living on the surplus produced by farmers and fishers and supplied with materials brought from afar. The Harappa relics can in fact only be products of specialized craftsmen—copper-smiths, potters, weavers, carpenters, jewellers, engravers. The workshops of such specialists have also been found—at Chanhu-daro Dr Mackay came upon a bead-factory replete with tools and products.⁵ Trade brought the craftsmen deodara wood from the Himalayas, copper from the north-western mountains, tin perhaps from Drangiana, lapis lazuli from Afghanistan, amazonite from the Nilgary Hills, and so on. Even for food citizens were not dependent on the products of adjacent fields ; dried fish was imported into Mohenjo-daro from the Arabian Sea.

Such an economy requires efficient means of transport. The Indus peoples, like the Sumerians, had mastered the secret of the wheel, still unknown in Egypt. They have left us models of carts and cars, among the oldest known representations of wheeled vehicles. Horse-bones have not been found in Harappa deposits, though some models from Harappa itself look very like Mongolian saddles. Boats are represented but seldom.

⁵ *Journal American Oriental Society*, LVII, 1-15.

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This civilization of course rested on a complex of scientific discoveries, and evoked more. What these were in detail and how far distinctively Indian is unknown. A literature existed, but its documents have perished. Its script is attested only by brief and undeciphered inscriptions on 'seals'. A system of metrology was essential in such an economy. A scale of measurements divided on a decimal basis and actual weights survive. The arithmetic and geometry that combined these have been lost. In chemistry the Harappa citizens were on a par with the Sumerians. They knew, for instance, the value of the alloy of tin and copper and were ahead of Mesopotamia in the use of glazes.

An original art reveals the spirit of the Indus cycle. Decoration is best illustrated by the pottery. The vases, wheel-turned and shapely, are covered with a rich red slip on which designs are executed in black paint. Repetition patterns, such as intersecting circles, envelop the whole vase surface as with a richly embroidered shawl. The style is unique. In small fragments examined close up its effect is uninspiring. On a large vase, however, it is superbly effective. It surely implies a no less splendid but lost textile art.

Motives taken from nature—conventional pipal trees, peacocks and even men—were sometimes combined with repetition patterns in ceramic decoration. But representative art is seen to greater advantage in glyptic and statuary. The so-called seals of glazed steatite probably served as talismans, since sealings stamped therewith are missing. Be that as it may, they are superbly engraved with figures of animals as well as with inscriptions. The beasts are portrayed with lively realism, but this realism is subjected to the canons of an unmistakable and dignified style reflecting the traditions of an individualized school.

In sculpture several heads from statuettes, that if completed might have stood 30 inches or so high, compare favourably with archaic Sumerian figures and as portraiture might be ranked with the statues of Gudea (*c.* 2300 B.C.) but hardly on a par with the masterpieces of Old Kingdom Egypt. Two mutilated limestone torsoes from Harappa and a little bronze dancing girl from Mohenjo-daro (PLATE IV, 2) are modelled with a liveliness of attitude, and the musculature and contours of the bodies delineated with an attention to detail and verisimilitude, found nowhere else before classical Greek times. Indeed so modern is the poise and treatment that the sculptures have been attributed to the Greco-Bactrian age. Admittedly the conditions of the discoveries are

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inconclusive. But Sir John Marshall has adduced technical grounds for believing them of Harappa age.

His view seems intrinsically probable. The liveliness of the representations is in striking contrast to the hieratic stiffness that affects even the finest efforts at portrayal of the human form under the Oriental monarchies. That may be merely a reflexion of social conditions under which the artists' objects were not viewed through priestly spectacles as necessarily superhuman. The men are treated in precisely the same spirit as the beasts depicted on the seals, or so beautifully modelled in faience.

Harappa art is in any case distinctively Indian. It is one proof of the specifically Indian aspect of the whole civilization. The latter represents a nice adjustment to the Indus environment, reflected in ideology as in material culture. And it was an enduring adjustment; much of the Harappa culture survives in India to this day. For instance a feature of the modern bazaar is the water-stall where you drink your draft from a clay cup which you then break—like the paper cups provided in American trains. In the bazaar quarter of Mohenjo-daro the remains of such a stall were found, its floor littered with the broken cups (PLATE II). Dr Mackay⁶ has shown in detail how the technique of the local potters is identical with that of the craftsmen who made the Harappa vases and how the modern village carts with solid wheels turning with the axle agree with those modelled for us in Harappa times.⁷ Anticipations of modern Indian costume are to be seen in the absence of pins and in the immense number of bracelets in Harappa cities. An ivory comb from Mohenjo-daro can be matched in wood in any village today. And as to the Indus religion Sir John Marshall writes that it 'is hardly distinguishable from still living Hinduism or at least that aspect of it which is bound up with animism and the cult of Siva and the Mother Goddess'.

By Harappa times the Indus civilization was already specifically Indian. On the other hand the cities of this age were heirs to a cultural tradition by no means confined to India. Into the fabric of Harappa culture are knit strands from many lands which recur again in Mesopotamia and Egypt, in Crete and Anatolia. The common traditions, which, divergently elaborated and differently blended, underlie Indus and Sumerian civilization are particularly obvious. Both peoples built on the same foundations of primary economics. The same ceramic

⁶ JRAI, LX, 131-4.

⁷ *Antiquaries Journal*, 1929, IX, 27 f.

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techniques—red wares, grey wares, reserved slip wares—are common to both regions but the forms produced are different. In Mesopotamia and the Indus valley the smiths used similar processes but even the simplest tools and weapons assume distinct forms; an Indian chisel or spear-head can be distinguished at a glance from a Sumerian one. The same wheel was attached to vehicles of different form, and so on.

These common traditions must go back to a remote past and may be relegated to the fourth millennium B.C. or earlier. By the third millennium these two areas confronted each other as civilizations as distinct as India and Rome at the beginning of our era. The extensive trade of the Harappa period can be deduced from the variety of exotic products found in the Indus cities. The extension of that trade to the Tigris-Euphrates valley is proved by a multitude of Indian manufactures found there.⁸

Seals of the distinctive Harappa type and made of the usual glazed steatite have been found at several Mesopotamian sites. Seventeen were recovered at Ur alone.⁹ Most must have been transported ready made from India, but one, engraved with cuneiform characters, might have been made by an Indus craftsman working in Mesopotamia. Imports of this kind are particularly numerous in the Diyala region, where a caravan route across Iran descends into the valley. From Eshnunna Dr Frankfort¹⁰ recovered a cylinder seal of Mesopotamian form but engraved with elephants (PLATE III) quite in the Indus style and apparently made of glazed steatite, kidney-shaped inlays such as were common at Mohenjo-daro, knobbed pottery, quite alien to Mesopotamia but popular in the Indus valley, and etched carnelian beads. All these imports were found in a house of the Sargonid period, 2500 to 2400 B.C. on the short chronology.

The etched carnelian beads are made by a very curious process. So many were found at Ur that Beck¹¹ believed they were manufactured there. But they are common in India too, especially at Chanhudaro,⁵ and there is one from Shah-tepe on the Turcoman steppe.¹² They are decorated with repetition patterns so popular in Harappa art. I believe the centre of fabrication to have been the Indus valley; if there were a workshop at Ur, it would have been manned by Indus artisans.

⁸ ANTIQUITY, 1931, v, 459 ff.

⁹ Gadd, *Proc. British Academy*, 1933, XVIII.

¹⁰ *Annual Bibliography of Indian Archaeology*, 1932.

¹¹ *Antiquaries Journal*, 1933, XIII, 384 ff.

¹² *Acta Archaeologica*, 1935, VI, 42.

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Indian exports in Mesopotamia are thus well established. Mesopotamian imports in India are less common. The fragment of a stone vase from Mohenjo-daro is certainly an instance. An exact parallel comes from Susa in Elam.¹³ Then there are two or three cylinder seals from Harappa cities ; their form is Mesopotamian, but the technique and the themes are Indian ; they result from the borrowing of Mesopotamian ideas in the Indus valley. Various artistic devices and religious symbols point in the same direction. The favourite Sumerian motive of a hero between two beasts is reproduced on a seal from Mohenjo-daro, but not only is the vehicle of the design local ; Indian tigers replace the Mesopotamian lions. Such instances suggest that ideas were transmitted as well as concrete objects. A convincing case is furnished by a vase from Tel Agrab on the Diyala¹⁴ which seems to depict in a truly Sumerian milieu an Indus cult scene (PLATE IV, 1) with the bull and manger so frequently repeated together on Indus seals.

The transmission of ideas, thus documented in the sphere of art and of religion, is an inevitable consequence of the conditions of international trade in the ancient east. If caravans traversed the rugged mountains and desolate wastes between the Tigris and the Indus, there must have been merchant colonies at either end comparable to the English colonies at Porto or Istanbul today.

Intercourse between India and the West did not cease with the Harappa phase. That brilliant civilization collapsed at an undetermined date and for unknown causes. It was replaced at Chanhu-daro by a different culture, represented also at Jhukar¹⁵ and named after the latter site. The Jhukar economy was formally urban—people still lived in cities—but judging by architecture was poorer. The cities were still inhabited by specialized craftsmen : potters used the wheel and carried on much of the Harappa tradition even in decoration. But the designs are now polychrome, and despite reminiscences of older motives we can recognize similarities to the ceramic art of the provincial settlements in Baluchistan discovered by Sir Aurel Stein. Connexions still further west are discernible.

The rectangular seals of glazed steatite are replaced by round bead-seals of baked clay and a few button-seals of stone. These are decorated not with lively pictures of animals but with geometrical designs. Both the forms and the motives of the Jhukar seals recall

¹³ *ANTIQUITY*, 1932, VI, 356.

¹⁴ *Illustrated London News*, 12 September 1936.

¹⁵ *Memoir Archaeological Survey of India*, 48 (1934), 10.

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those current on the Iranian plateau—as represented for instance in the first settlement at Tepe Hissar near Damghan. Pins, unnecessary in Harappa times, were used for fastening garments in the Jhukar period at Chanhudaro. Among those recovered is the pin with double-spiral head. This type¹⁶ was worn also at Anau in Turkestan; in the second and third periods at Tepe Hissar and at a whole series of sites in northern Iran and Anatolia and as far west as Greece and Bulgaria, all dated rather vaguely to the latter half of the third millennium B.C. Plotted on a map the distribution of such pins is seen to coincide with the ancient caravan route from inner Asia across Anatolia to the west. Traffic was already following that route about 2000 B.C. and was affecting India in the Jhukar period.

In the same context another discovery may be noted though its relevance is not so certain. In the debris overlying the Harappa ruins at Mohenjodaro but not explicitly associated with Harappa or other relics, Dr Mackay¹⁷ found an axe-adze, a peculiar type of implement particularly common in Hungary, but found also in Sardinia, Greece, Palestine and the Caucasus. The Indus specimen is quite isolated in India and only in the most general way like the European forms; but it is practically identical with some found in Hissar III, and there moulds for casting the type were also discovered. The relation of the Iranian and Indian axe-adzes to the more westerly ones need not be discussed here—the axe-adze might arise anywhere on the fringe of the Sumerian metallurgical province by combining in a single casting the two types of Sumerian shaft-hole axe. The isolated specimen from Mohenjodaro, identical in form with those from Tepe Hissar, is, if not an import from Iran, a local product in the Iranian tradition. It testifies once more to influence on India from the northwest in the post-Harappa period. Evidence in the same direction is forthcoming from the Indian borderlands.

In Baluchistan and on the Makran coasts Sir Aurel Stein laid bare the ruins of provincial and barbaric settlements, some at least contemporary with the Harappa cities. In the one of these, the township of Shahi-tump in Makran, typical Harappa pottery is not uncommon. But according to Stein,¹⁸ graves had been dug into the ruins of the townships, thus dated to the Harappa period. These intrusive graves

¹⁶ *Liverpool Annals of Anthropology and Archaeology*, XXIII, 118 and pl. LII, c.

¹⁷ *Annual Report Archaeological Survey of India*, 1927-8, 76.

¹⁸ *Memoir Archaeological Survey of India*, 43, 88 ff.

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differ radically both in ritual and furniture from burials of the Harappa period but agree surprisingly with those of Susa I in Elam; the funerary vases for instance in technique, form and ornamental repertoire belong to the Susa I tradition, however much the execution of the patterns fall short of the mastery achieved at the more western site. The burials at Shahi-tump would therefore seem to be due to intruders from the west into a province of Indus civilization in post-Harappa times.

It is just possible that the intruders reached the Indus valley itself. Above the Jhukar level at Chanhudaro Mackay found several vases of fine grey ware, decorated with incisions, not painted. One of these was a triple vase precisely like one from the cemetery at Shahi-tump.

During the period of Indus civilization's decay contacts with the west are still perceptible but now with the highland of Iran rather than with the civilized Tigris-Euphrates valley. They find expression in what are vaguely termed influences, but influences, it would seem, *from* the West. Are they connected with, do they at least foreshadow, the still inferential Aryan invasion of India? If so the influences of the invasion do not appear in the archaeological record as enrichments of, or stimuli to, native Indian culture. Materially they seem lethal rather than vivifying. Previously India had been making positive contributions to the cultural traditions of Hither Asia. In the sequel she would have received little material help from that quarter. The Aryans would be disclosed as the destroyers rather than the creators of Indian civilization.

After the historical daylight of the brilliant Harappa age twilight descends upon the Indus valley. From the archaeologist's standpoint the Aryan rishis sang their Vedic hymns out of a prehistoric night.

The Rise and Decline of the Medieval Community

by R. R. DARLINGTON

1. THE CONSTITUTIONAL HISTORY OF ENGLAND FROM THE ENGLISH SETTLEMENT TO 1485. By J. E. A. Jolliffe. A. and C. Black, 1937. pp. 524. 15s.
2. THE FEUDAL MONARCHY IN FRANCE AND ENGLAND FROM THE TENTH TO THE THIRTEENTH CENTURY. By Ch. Petit-Dutaillis. Kegan Paul, 1936. pp. xx, 421. 16s. Translated from the French, *La Monarchie Féodale*, published in 1933.
3. LIFE ON THE ENGLISH MANOR: A STUDY OF PEASANT CONDITIONS, 1150-1400. By H. S. Bennett. Cambridge Press, 1937. pp. xviii, 364. 16s.
4. THE STRIP-MAP OF LITLINGTON. Ordnance Survey, Professional Papers, New ser., no. 17, 1937. 2s 6d.
5. A HISTORY OF BRITAIN. By E. H. Carter and R. A. F. Mears. Oxford, Clarendon Press, 1937. pp. 1050. 10s 6d.

THE accumulation of specialized work and the publication of more texts calls for the constant modification of current conceptions of the history of medieval England. Of the means of achieving this readjustment the most satisfactory is probably the lecture, which affords scope for the individual interpretation of development over a wide field and yet does not lose its flexibility or arrogate to itself an authoritative character to which, owing to its very nature, no general survey can rightly lay claim. It is not suggested that the twentieth-century medievalist should ignore the printing-press altogether, but there is much to be said for concentrating on the printing of such works—in the main, editions of texts—as may possibly be of some value to future generations of scholars. To the demand for ‘brighter’ history it is well to turn a deaf ear, but the exigencies of time to some extent justify the publication of re-interpretations by those competent to undertake them, whether such text-books assume the form of the co-operative work with almost every chapter from a different pen, the many-volumed history with an expert in charge of each century or so, or a general survey of one or other of the compartments into which history is commonly divided. Of these the last might be deemed the least satisfactory,

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for the treatment of 'constitutional', 'economic' or 'ecclesiastical' history as a self-contained entity may seem a vicious and outworn practice, leading inevitably to distortion. It would however be discourteous not to admit the stimulating character of a good specimen of this type of re-interpretation such as Mr J. E. A. Jolliffe's *Constitutional History*. In such a work the author's presentation is more interesting than the matter. Mr Jolliffe's approach is in a sense that of the philosopher, and the problems confronting the student of constitutional history do not become less abstruse at his touch. He speaks of the 'political theory' of men who were, we may think, innocent of anything so subtle. However, the remark that the interest of Stephen's reign lies 'in the assertion of what may be called the alternative political theory of the day, that which had played a great part under the Conqueror, had been forced into the background by Henry I and which now imposed itself more freely than at any time in English history' is but another way of putting the commonplace that baronial policy in the Anarchy was the natural reaction against the oppressive centralization of Henry I and of expressing the now familiar view that their 'aggression' was largely the reassertion of rights enjoyed in the first two Norman reigns.

The earliest phase of the Anglo-Saxon period, where conjecture is easily presented as fact, is not unnaturally viewed by Mr Jolliffe in the main from a Kentish angle, and it would seem that the trend towards unification is less important from a 'constitutional' than from a 'political' point of view. In the later Saxon period, the tenth and eleventh centuries, we have one of these formative periods when institutions undergo marked change. The significance of some of the developments of this age in which the origin of so much of the later machinery of government is to be found, is well brought out in this survey.¹ Mr Jolliffe supports, as indeed anyone who understands the evidence must, the view that English society on the eve of the Conquest was not feudal in character. To rights and conceptions inherited

¹ Some of the defects of Mr Jolliffe's chapters on the Anglo-Saxon period are pointed out by Miss F. E. Harmer in her extremely learned and highly important work on *Anglo-Saxon Charters and the Historian* (Manchester University Press, pp. 31, 15 6d, reprinted from the 'Bulletin of the John Rylands Library', October 1938). Miss Harmer gives reasons for arriving at the conclusion that 'so far as Anglo-Saxon texts are concerned, it is not easy to feel full confidence in his use and interpretation of them'. Some of her criticisms of Mr Jolliffe's use of charters and writs may be cited to support the foregoing remarks regarding the relative urgency of critical editions of texts and works of a general character.

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from pre-Conquest times rather than to the newly imported feudal institutions with their centrifugal tendencies, the strength of the Norman monarchy—perhaps not so great as is sometimes supposed—was largely due. Without doing injustice to Henry I, it can still be maintained that a new epoch opened in 1154. The reign of Henry II, when so much was refashioned or newly created, is the most formative in our history; and the interest of the thirteenth century lies to a great extent in the perfection and modification of the machinery of government which Henry and his ministers evolved.

The achievement of this remarkable reign is the more striking when it is recalled that, owing to the claims of his continental possessions, the king spent not much more than a third of his time in England. Ruling a vast and heterogeneous dominion, 'Henry II and Richard Coeur de Lion had the cosmopolitan political ideas, tastes and instincts of an emperor rather than a king'. Brilliant administrator as he was, Henry 'did not attempt to coordinate the laws and the administration of his Continental possessions', and Normandy is the only province regarding the government of which we have detailed information. But if Normandy alone had a strongly centralized administration like that in England, the king was no mere figure-head in his other French fiefs. The ambitions of Henry and Richard extended beyond their already vast dominions and in the opinion of the distinguished French historian M. Petit-Dutaillis, who has made an illuminating study of the French and English monarchies, the Capetians, hemmed in on all sides, would have succumbed to the Angevins but for the ability of Philip Augustus. The power of the house of Anjou undoubtedly retarded the rise of a centralized monarchy in France, but it does not alone explain the contrast between England and France in the twelfth century. On the immediate predecessors of Philip II, M. Petit-Dutaillis's judgment is somewhat harsh—'These men (Philip I, Louis VI and Louis VII) as a result of their weakness or the simplicity of their intellect were subject to the influence of those around them—their wives, the barons, and prelates who thronged the Court, and minor officials equally with the more important'. Judged by the standard of the first three Norman kings of England and the Angevins, the Capetians were indeed weaklings, but the wisest were perhaps those who attempted least. The task of creating a centralized monarchy was far more difficult in France than in England. In France there was no strengthening of royal power in the tenth century and the first half of the eleventh as in England and Germany. William the Conqueror found in being a kingdom

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already united and possessing a highly developed administrative system capable of unlimited growth. Though he himself created feudal lordships not unlike those on the Continent, the monarchy in England, partly through rights and institutions inherited from the Anglo-Saxon kings, maintained its supremacy. The recognition of the early Capetians, even in the remoter parts of France as 'at least *de jure* if not *de facto*' kings did not give them effective power over their feudatories. Their demesne and resources in general were not commensurate with their pretensions, and the most valuable right which they had inherited from the Carolingians was perhaps their control over many sees beyond the limits of their demesne as well as within it.² In the circumstances, it is not surprising that centralization was so much slower than in England. It is arguable that John's loss of Normandy and other French provinces was ultimately beneficial to England. Certainly it was an event 'of extreme importance in the history of the French monarchy the prestige and resources of which were suddenly doubled'. The French kings profited, not least in the sense that the danger of English interference was removed, by the troubles of Henry III's reign. The kingdom was gradually transformed and 'the victory of the monarchy over anarchy was almost complete by the end of St. Louis's reign'—about a century later than in England. There is now a somewhat closer resemblance between the two realms whose kings 'stretch their arms beyond feudal bounds': in each the monarchy is 'above feudalism'. In England the magnates seek not to destroy but to control the administrative machine and the king is intent upon establishing closer contact between the Crown and the mass of the free men of the land. He asserts his right to impose taxation on all his subjects, claiming in the early thirteenth century 'a width of consent which it had probably never received', and from time to time affords the feudal council with popular representatives. 'Speaking with the widest latitude', says Mr Jolliffe, 'the reign of Henry III had seen the collapse of feudalism as the basis of the practice and theory of political life', and the period

² The passage in the translation 'In the South, in Brittany and Normandy, the nomination of bishops *fell to the Capetians* but in the ecclesiastical provinces of Reims, Sens, Tours, and in the centre of France four archbishops and twenty bishops were at the disposal of the king' does not make sense. Clearly '*échappait aux Capetiens*' of the original should be translated 'was lost to the Capetians'. From time to time the translation leaves something to be desired and it should have been revised by a historian. The retention of French forms of familiar names—Plantagenet, Richard de Lucé, Raoul de Wanneville (*recte* Warneville), Pierre de Blois and Gervais of Canterbury, for instance—is irritating.

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which follows (1272-1377) is for him the 'Parliamentary Monarchy'. The importance attached by modern scholars to the evolution of parliament—an intricate and elusive subject—is not wholly retrospective, but the history of the administrative departments, so easily followed in the monumental work of the late Professor Tout, is of greater moment. The rise of parliament may have led to a change of outlook, but it is hardly true that in the fourteenth century 'reform no longer proceeded by violent crises divided by long periods of years, but by the steady pressure of successive parliaments'.

Either because the Commons now had more opportunity to make complaints, or in consequence of a real weakening of effective central control, there are everywhere signs in the later middle ages of the deterioration of public order. The parliament rolls reveal that from the early fourteenth century 'the honesty of the provincial courts was declining and that the law itself was becoming a weapon in the hands of the unscrupulous'. Still more serious was the undermining later in the century of orderly government by the growth of the 'new feudalism' reproducing the worst features of the old 'without its stability and almost without restraining rules'. Like Stubbs, Mr Jolliffe makes Parliament the focus of his study of the later middle ages, but his conception of 'constitutional history' is narrower than that of the great Victorian. There is little in his survey regarding the relations between Church and State which might reasonably be regarded as an aspect of fundamental importance. Between Henry II's submission to Alexander III at Avranches in 1172 and the Reformation Parliament, Englishmen acknowledged not one supreme legislator and one supreme judge but two, and without a study of the interaction of church and state a survey of constitutional development can hardly be complete.

The Church is rightly brought into Mr H. S. Bennett's recent study of peasant life in England between the middle of the twelfth century and the close of the fourteenth. The attitude of the rustic to his parish priest, to ecclesiastical institutions in general and to the religious beliefs of the time must necessarily however be a matter of conjecture. That he lived in a dark world where the boundary between religion and magic was ill-defined, no one can deny, but it is to be regretted that miracles of English saints are not occasionally cited, instead of the inevitable Caesarius of Heisterbach, to illustrate the outlook of the age. They might for other reasons be given a place, though a minor one, among the materials of the economic historian. More

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complete knowledge of economic conditions in rural areas in the middle ages is being attained gradually as more records are made accessible through the labours of editors and the readiness of local societies to undertake the publication of charters and extents, whole cartularies, assize rolls, rolls of manorial courts and other local records. It is now possible to give a more complete if not a more satisfying picture of rural England, a task which Mr Bennett has attempted for the benefit of a wide public. Our records have thrown welcome light on the administration of individual manors and groups of manors, on account-keeping, the duties of manorial officials—supplementing and acting as a check upon the famous treatises on estate management written in the thirteenth century—on the services due from unfree tenements, on the taking of reasonable and excessive heriots and a host of other matters. As the details accumulate the impression of infinite, almost bewildering, variety is strengthened, and it seems somewhat bold to speak or write about 'Life on the English Manor'. Mr Bennett aims at 'building a little further upon the foundations laid by Maitland and Vinogradoff' who, we are told, 'being lawyers and philosophers naturally stressed the legal and abstract point of view'. This is no doubt partially true, but it cannot be admitted that they 'were often forced to ignore the actual in order to establish the theoretical'. They paid little attention to what might be termed 'every-day life'—of which scarcely anything is known. Next to nothing can be said about the dwellings of the peasantry, and Mr Bennett has to resort to Becket's biographer Fitzstephen when he comes to write about pastimes, and the Londoners of the twelfth century were not peasants.

The curious idea—of foreign origin—that economic history has too long suffered through 'the legal angle of approach to economic facts' is popular at the moment among professional exponents of economic history—from whom the author has doubtless derived it—but it is none the less without foundation. Fortunately, Mr Bennett writes delightfully simple and straightforward prose, entirely free from those foreign—particularly German—phrases with which the economic historian of today so often lends distinction to his work. The reaction against the so-called legal interpretation of the evidence seems to be responsible for the main weakness of this useful piece of work, for it is to be regretted that the author has not attached more importance to the distinction between the free and the unfree. Some of the most important work of recent years on rural life in medieval England is undoubtedly that which has revealed the existence of a large free element among the

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peasantry and of villages which were but partially manorialized or 'unmanorialized'. To such studies the author himself makes reference, yet he had very little to say about the free element, and when he uses the term peasant he generally means *unfree* peasant. Large manors inhabited mainly by villeins were very common, particularly on the estates of great churches, but Mr Bennett has allowed himself to be too little influenced, in his presentation of manorial life, by varieties of social structure. His own excellent description of the burdens resting on servile tenants—heavy labour services taking up several days of the week, tallage, mill-suit, liability to serve as reeve, and many more—in itself illustrates the importance of the problem to which Maitland and Vinogradoff are held to have given too much attention, for there was a large mass of free peasants subject to none of these. In spite of the author's incidental remarks, this book preserves too much the old idea of uniformity. Though he speaks at length of carrying services, and in another connexion refers to the illuminating foundation charter of Revesby abbey, we find in his view that the peasant did not normally travel outside a radius of a few miles of his home, an echo of the old idea that manors were self-contained and self-sufficing units.

The working of the system of agriculture wherein the village arable was divided into two or three fields is well described by Mr Bennett in his chapters on 'The Manor and its cultivation' and 'The peasant's year'. It may not be inappropriate here to protest against the common application of the term 'Midland System' (apparently invented by Mr H. L. Gray) to a system of agriculture prevailing in a wide stretch of country extending from the Tyne—or the Tweed—to Hampshire. Still more misleading is the use of the phrases 'the midland manor' and 'the midland village' to indicate the manor which coincided with the whole village cultivated in this way; for in the true *Midlands* it is not uncommon to find in a single village strips belonging to different manors. The open fields whose almost total disappearance has cost the English countryside so much 'both in loss of economic opportunity for its workers and in the loss of a sense of personal responsibility to the village community for its social institutions',³ have even in regions still devoted to arable farming left traces which a hundred years and more of ploughing on a wholly different plan have not sufficed to obliterate. Besides lynchets there are traceable in some parts of the country raised headlands of the type recently described by Mr O. G. S. Crawford in

³ C. S. Orwin, *Econ. Hist. Rev.*, vol. VIII, p. 134.

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his illuminating introduction to the *Strip-map of Litlington*. 'These are long low banks some 25 yards in width rising gradually to a maximum height of from one to two feet. Superficially they resemble the ploughed-over remains of the causeway of a Roman road for which they have sometimes been mistaken'. Such headlands served the double purpose of a place where the plough-teams could turn and a cart track. In an area 'extending from Cambridgeshire in the northeast through Hertfordshire, Bedfordshire, Buckinghamshire and Oxfordshire across the Thames into Berkshire' they are still visible on the ground and from the air, even though the modern field arrangements bear no relation to the system to which they belonged. The fortunate survival of a strip-map of Litlington on the border of Cambridgeshire and Hertfordshire (made in 1804 and reproduced in facsimile in this pamphlet), establishes the identity of the headlands traceable in this parish—some can be seen very easily in the air-photographs published with the monograph—with the tracks called 'High Joints Way', 'High Middle Path' and the like before enclosure. This is of course only one type of headland, for the headland must normally have been cultivated, though being ploughed last it was less valuable than the other strips. The grant of a *forera* is very common in for example the thirteenth century, and it would be pointless to buy or sell, if not to give away for the soul's health, a portion of a path or cart-track.

Our debt to the Ordnance Survey for the provision of maps of sites cultivated on the Celtic and 'open-field' systems of agriculture, of Roman and early Saxon Britain, and the reproduction of the fourteenth-century map of England in the Bodleian Library is one which no scholar will under-estimate. It is in general easier now to procure good maps and illustrations, reproductions of drawings in early manuscripts, photographs of antiquities and plans of ancient buildings. By using such material the task of 'humanizing the treatment of the subject' which faces writers of text-books for schools is to some extent simplified. Mr Carter and Mr Mears, authors of *A History of Britain* intended to cover the various school certificate and matriculation syllabuses, are alive to their opportunities and have chosen their illustrations with care. The pupil who does not find this book interesting to read and is not drawn to make a careful study of the maps and illustrations must indeed be dull. To write a volume beginning with the age when men eked out a wretched existence with the aid of stone implements and ending with the rise of post-war dictatorships and other developments which may in time complete the circle

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is no easy task, and in view of the wide scope of the book the section devoted to the Middle Ages is not unduly short. Naturally such a work abounds with generalizations of a questionable nature, and it would be easy to quote passages where the claim that 'the results of the latest researches' are incorporated is hardly justified. However, by weaving into the general narrative observations on such matters as the rise and character of medieval towns, the wool trade, the cloth industry, schools and universities and 'daily life in a monastery', the authors contrive to give their readers a fair idea of the nature of the medieval community at large.

The 'Gododdin' of Aneirin

by KENNETH JACKSON

HISTORIANS have known the 'Gododdin' ever since Skene's edition and translation in the *Four Ancient Books of Wales* (1868), and have realized the possibility that its claim to be the work of a sixth-century British poet may be in some form true. The text however was so obscure and in parts corrupt, and hence Skene's and other translations so plainly unreliable, that few have cared to deal boldly with it. Now comes Professor Williams' life-work,¹ his edition of and introduction to the Book of Aneirin, in which he establishes as satisfactory a text as is ever likely to be made, and solves the great majority of the difficulties of interpretation.² The importance of this book cannot be overestimated, and since the whole is written in Welsh a full description is necessary.

The 'Gododdin' is an elegy on the British warriors killed at Catraeth, not a narrative poem, so that it does not tell any connected story. We learn however that there was a chief called Mynyddawg Mwynfawr who ruled over the Britons (called always *Brython*, never *Cymry*) from a city called Din Eidyn or Caer Eidyn. The first is evidently Edinburgh (Welsh *Din Eidyn*=English *Edin-burgh*=Gaelic *Dún Eðin*); the second may be Carriden at the eastern end of the Antonine Wall; Eidyn is probably the name of the whole district. His land and people are called *Gododdin* (in Nennius *Guotodin*, where Cunedda came from), from a British **Votodīnī* appearing in Ptolemy

¹ *Canu Aneirin*, by Ifor Williams. Cardiff, 1938. Pp. xciii and 418. 15s.

² As usual he is careful with emendations. P. 1, l. 18, *kwl y uot a dan vein*, one might emend *a dan vein*, as *brein* has occurred in rhyme just before and *mein* gives better sense; cf. *Canu Llywarch Hen* p. 14 vv. 20, 27. On l. 773 he guesses *aer* 'slaughter' for *hair*; but *hair* is actually the Old Welsh form in the Glosses, gl. *clades*, see Lewis-Pedersen, *Comparative Celtic Grammar*, p. 120. P. 298-9, on the *-anawr* termination, contrast op. cit. p. 307 note 1, which might have been mentioned. The meaning 'so' for *hu* seems to be laboured in some places where it will not bear it, perhaps in the belief (*Canu Llywarch Hen* p. 131) that it is cognate with English *so*; but *so* had initial *sw-*, which excludes this. P. lxxxiii, quoting A.S. *Eoforwic* as a 7th-cent. example of Welsh mutated *-b-* borrowed as *-v-*, he has misunderstood Ekwall; *Eoforwic* occurs in the Chronicle s.a. 644 and other 7th-cent. dates, but that is of course a very different thing from being the 7th-cent. form.

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as Otadēnoi ; the district reached from the head of the Forth (*Manaw Gododdin*) through the eastern Lowlands towards Hadrian's Wall. Mynyddawg assembled a picked war-band (*teulu, gosgordd*) of Gododdin men, and adventurers from all parts of the British world, including Aeron (? Ayr), Elmet in the Pennines, and even North Wales, to the standard number of three hundred. For a year he feasted them on mead and wine in the halls of Eidyn, and then sent them against the Northumbrians, who are called Saxons (*Saeson*), 'men of England' (*Lloegrwys*), and more specifically 'men of Deira and Bernicia' (*Dewr a Bryneich*). These are a vast host loosely described as a hundred thousand. In the battle all but one of the Britons were killed. The place is called Catraeth, which has usually been identified with Catterick (Ptolemy *Caturhactonion* ; Ant. It. *Cataractone*, abl. ; Bede *Cataracta*, and *Cataractone* abl.) ; there are certain philological difficulties about this, but Professor Williams solves them finally and satisfactorily, and brings strong arguments in support of Catterick as the site of the battle.

As to the background of life and culture, the warriors feasted in the hall by rushlight, drinking wine out of glass cups, and mead and beer, in return for which it was their duty to fight faithfully their lord's battles, which was called 'paying for mead'. Williams compares the Finnsburg Fragment l.41, *ne naefre swanas swetne medo sel forgyldan donne Hnaefe guldán his haegstealdas*. They rode to battle and apparently fought on horseback (a Roman practice), but some were evidently infantry (e.g. l.1295) though Williams seems to ignore this. They were dressed in mail-coats (also a Roman relic), carried whitewashed shields, wore gold collars and perhaps diadems and amber beads, fought drawn up in ranks with swords and spears and throwing-javelins ; arrows are not mentioned. They were Christians, going to church to do penance and laying gifts of gold on the altar ; the poet prays that their souls may go to Heaven in union with the Trinity. The enemy however were heathen (*gynt*). Typical passages are :

v.xxxi. The men hastened out, they leaped forward together ; short were their lives, drunk over the clarified mead, the war-band of Mynyddawg famous in battle stress ; their lives were payment for their feast of mead. Caradawg and Madawg, Pyll and Ieuan, Gwgawn and Gwiawn, Gwynn and Cynfan, Peredur of the steel weapons, Gwawrddur and Aeddán, one who rushed forward in battle among broken shields ; and though they were slain they slew ; none returned to their lands.

v.viii. The men went to Catraeth, vigorous was their host, the pale mead was their feast and their poison ; three hundred fighting in ranks, and after the shout of battle there was silence. Though they went to churches to do penance, true is the tale, death overtook them.

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v.XXXIII. The men went to Catraeth in a battalion with the war-cry, a force of steeds and blue armour and shields, shafts aloft and sharp spears, and shining mail and swords. He led the van, he penetrated through armies, there fell five fifties before his blades, Rhufawn the Tall, he presented gold to the altar, and gifts and fine presents to the poet.

v.LXXXII. Pitiful I think it after the toil of battle to endure the pang of death through suffering; and a second heavy grief it is to me to see our men falling headlong; and long sighing and misery after the fiery men in the clodded earth—Rhufawn and Gwgawn, Gwiawn and Gwlyged, men of most valiant station, strong in strife; may their souls after battle get welcome in the land of Heaven, the dwelling-place of plenty.

Vivid lines such as 'He used to strike with a fierce bloodstained blade, as when a reaper strikes in stormy weather' (v. XXVI); or 'Though we drank bright mead by the glow of rushlights, though its taste was good its curse was long-lasting' (v. xv); or 'His sword rang in the heads of mothers' (v. XXVII); or 'The heathen, the treacherous men of Deira, used to ask whether there ever came from the Britons a man better than Cynon, the serpent stinging the foe'.

There are striking analogies with Anglo-Saxon heroic poetry that might have been mentioned, apart from 'paying for mead'; for example the many synonyms for weapons and battle; the fence of shields, *ysgorva ysgwydau* (l.122), exactly parallel to the familiar *bordweall* and *scyldburh*. 'They were feasted together for a year over the mead, great were their pledges' reminds one of the typical Anglo-Saxon heroic feast scene and the warrior's boast there, his *gilp-cwide* (cf. Beowulf ll.639-40). In some ways the subject is peculiarly like that of the Battle of Maldon, which tells how the English made their gallant effort to drive off an over-whelming force of Viking pirates in 991.

The supposed date of the battle of Catraeth can only be fixed indirectly, though according to Nennius Aneirin flourished in the middle of the sixth century. Among the many names given in the 'Gododdin' only one can be identified at all certainly, namely the Cynon son of Clydno who was the sole survivor. Clydno of Eidyn and his son are known figures in early Welsh genealogy and tradition, and according to these Cynon should have flourished around 600. As for the others, several names occur which are found in the early Northern genealogies about this time, but there is no certainty that they are the same people. A Madog of Elfed is mentioned, and since the kingdom of Elmet was destroyed by Edwin soon after his accession this suggests that the battle was before 617. At any rate it must have been before Edwin extended Northumbria to the Forth. The fact

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that Urien of Rheged and his sons, the great British enemies of the Bernicians in the last part of the sixth century (Nennius and early Welsh poetry), are nowhere mentioned may mean again that Urien was already dead; he was alive as late as 585, as he fought against Husa (Nennius). Thus the period represented by the 'Gododdin' seems to be narrowed down approximately to the reign of Aethelfrith, 592-617. After the victories of Urien, and before Aethelfrith had had time to consolidate his power in Bernicia, an attack such as that described in the 'Gododdin' could perhaps afford to ignore Bernicia and make its objective the growing and spreading colony of Deira. Archaeological evidence shows that while the Bernician settlements were few in the late sixth century, Deira was much more strongly developed, and the Angles were pushing up the Vale of York as far as and past Catterick. It looks like a desperate attempt to drive back the growing Deiran power while Bernicia was still weak; such an attempt would have been impossible after Aethelfrith had annihilated the Scots and Strathclyde Britons under Aedhán at Degasstán in 603 and united Northumbria in 605, so that the last years of the sixth century are the most likely date for the battle. On the other hand, though the identification of Catterick and Catraeth is philologically quite possible, one must point out in disagreement with Professor Williams that geographically there are difficulties.* Catterick is a very long way from Edinburgh; and moreover according to the early Taliesin poems Catraeth had been in the lands of Urien of Rheged, a kingdom comprising apparently southwest Scotland and northwest England; whether it reached as far as Catterick must seem doubtful. If not, Catraeth must have been somewhere further north, perhaps near the Roman Wall; and in that case the battle would be against the Bernicians, perhaps an endeavour to stem Aethelfrith's growing power just like Aedhán's similar disastrous failure at Degasstán. How well it would fit if we could assume that the two battles were one, that the Gododdin men joined their kinsmen of Strathclyde under Aedhán and were wiped out along with them at the place, clearly within the kingdom of Rheged, called by the English Degasstán (Dawston in Liddesdale). It is true that the poem gives no hint of any such alliance, but perhaps the poet was only concerned with his own men and took no count of any but the contingent from Gododdin. This would make such an attack by three hundred men seem a little less rash. However that may be, the approximate date of the battle seems fairly certain.

* See note at end.

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Professor Williams thinks that Mynyddawg's war-band of mailed cavalry was a conscious imitation of the Roman-inspired band of Arthur,³ who died according to *Annales Cambriae* only about fifty years before. It is curious that Mr Hodgkin thought exactly the same of Aedhán, who 'seems to have been inspired by the idea of being a second Arthur' (*Hist. Anglo-Saxons* 1, 197). In this connexion it is very significant that in the 'Gododdin,' ll.1241-2, a warrior is mentioned who 'glutted the black ravens on the rampart of the city, *though he was no Arthur*'. This is perhaps the most valuable evidence yet found for the historicity of Arthur; here we have him spoken of as a famous warrior within living memory of the (supposed) date of his (supposed) death.

So far it has been merely assumed that the 'Gododdin' is the work of a contemporary poet of the late sixth century. It remains to show that this is possible. The 'Gododdin' is found in a single MS of the thirteenth century in two contemporary hands, A and B. A gives the whole poem, in modernized spelling; and B adds forty-two verses, some of them variants on the A version and others fresh ones. These are mostly unmodernized, in the Old Welsh spelling of the ninth or even eighth century, so that B was copying from a MS as old as that; and one might add that the character of the textual corruptions seems to suggest that this was not the first written version. But both A and B show quite plainly very considerable oral differentiation and corruption within their respective texts; in other words, both have behind them a history of oral recitation and tradition, perhaps a long one. This fits in with the well-known scribal note on p. 28 of the MS, which makes it clear that the 'Gododdin' was a bardic classic regarded as absolutely indispensable for oral recitation at poetic competitions of the kind which are known to have existed in medieval Wales. The note itself seems not to be old, but the tradition it preserves may be ancient. Hence we can trace the written text of the 'Gododdin' definitely back to the ninth century and perhaps earlier; and can show that even at this early date it was already corrupted by oral tradition. The gap between the thirteenth century MS and the presumed date of Catraeth is therefore narrowed to a very small one; and the possibility that it is genuine seems much more than a pious hope.

Linguistic evidence supports it. The language is archaic and difficult. But more, Professor Williams has made a brilliant discovery

³ He compares Collingwood and Myres, *Roman Britain*, pp. 322-3.

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that will revolutionize our ideas about the development of Old Welsh. Before the old British grew into Welsh, certain consonant changes called the mutations came into existence, by which in certain positions an initial *t* for example became 'mutated', voiced to a *d*. In Welsh poetry *d* from mutated *t* alliterates with ordinary *d*, since they are the same sound. But Williams shows that while alliteration is frequent in the poem, it ignores these mutations; mutated *t* alliterates with ordinary *t*, not with *d*. Yet metre proves that the poem cannot be as old as British. What is the explanation? Williams argues that though the mutations must have begun in the British period, they went on developing afterwards for some time within the Welsh period. Thus *t* in mutated position must have become first a kind of half-voiced sound not a full *d*; after the loss of the British terminations which caused this change, and the coming into existence of Welsh, the sound, which was unstable, continued to develop towards and finally reached the fully voiced *d*. The 'Gododdin' must have been composed at a stage so soon after the change from British to Welsh that the mutated consonants still sounded more like, and so alliterated with, *t*, *p*, *c*, etc., rather than *d*, *b*, *g*, etc. If so, then it testifies to the extreme antiquity of the poem. In criticism of this one wonders how it was, if the mutated sounds were still so like the unmutated when the causes had ceased to operate that they sounded the same to the ear, that they should ever have developed any further with all the forces of analogy against it. It may be that actually they *were* already nearer to the fully voiced sounds, but that conservative feelings for 'good grammar' lagged behind the facts, as they always tend to do, and prevented the conscious recognition of this. The whole problem however cannot be separated from the question of alliteration in Irish, where analogous linguistic changes occurred. Here from the earliest times the rule was that mutated consonants could alliterate in poetry with unmutated, e.g. *d* arising from *t* by mutation (eclipsis) alliterated with ordinary radical *t*, not *d*. Since the mutations were not fully recognized in spelling (any more than they were in Welsh), and *d* from *t* was still written *t*, this has been dubbed 'alliteration for the eye'. Williams, rightly arguing that early Welsh poetry was meant to be heard, not read, refuses to allow the analogy with Irish, and holds that the Welsh alliteration must have been a phonetic fact, not an empty orthographical convention. He has been led astray by an unfortunate phrase. Of course the Irish alliteration is no more 'for the eye' than the Welsh; early (and later) Irish poetry was as much primarily a matter of recitation as Welsh,

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and its writing down is an irrelevant accident. For Irish the clue has been provided by Professor Bergin.⁴ He shows that the conditions of this alliteration are that there should be both close phonetic similarity though not necessarily identity, and the feeling that in its unmutated state the consonant is the same as the one with which it alliterates. This is like the Irish system of rhyme, whereby rhyming consonants do not need to be identical but only phonetically very similar. In early Welsh poetry much the same rhymes are found, and are called 'Irish' rhyme, may not the parallel hold for early Welsh alliteration also? That is to say, that at the time of the Gododdin *d* from mutated *t* was already a genuine *d* in sound, or close to it; but it could only alliterate with radical *t*, not *d*, because it was felt, consciously or unconsciously, that it belonged in origin to the former rather than to the latter. In time this feeling faded, overcome by the phonetic fact that both *d*'s were really the same sound; and the new rule took its place. Significantly, Williams himself points out that traces of the Gododdin type of alliteration are still found in Welsh as late as the ninth or tenth century in the *Armes Prydein Fawr*, where mutated consonants can still occasionally alliterate with unvoiced (Gododdin type) as well as with the voiced consonants (later Welsh type). If my explanation is true, Williams' discovery of this archaic kind of alliteration need not prove the 'Gododdin' to be so very ancient that it is almost as old as the British period. Yet the conclusion is, as before, that at any rate it is considerably older than the ninth century.

The claim for the actual authenticity of the 'Gododdin' rests finally on *a priori* arguments. The numerous people mentioned are almost entirely forgotten in later Welsh tradition, so that a forgery would be pointless. The whole tone of the descriptions is that of personal knowledge, and would have no significance if not contemporary. It is not a narrative about the past, or part of such a narrative, like the Llywarch Hen poems, but a long elegy on a number of very definite and specific people. A late inventor would have attempted to give details about the Saxon enemy and their leaders, as Nennius does, and the absence of these may perhaps imply, as the same feature is held to imply in the case of the Battle of Maldon, that the author was an eyewitness who had not had time to learn these details. Since the north Britons were fairly well cut off from their Welsh brothers by the time of the death of Cadwallon in 634, no one in Wales would know enough about ancient northern affairs much after that time to be able to forge

⁴ *Ériu*, IX, 82-4.

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such a poem, and it must have come south before then. On Cadwallon, Williams notes that the early Welsh elegy on his death, which he thinks may actually be contemporary, speaks of 'the tragedy of great and famous Catraeth' within fifty years of the supposed date of the battle.

These arguments are inconclusive perhaps; but if along with them one considers how well the poems fit into the history of the last years of the sixth century and beginning of the seventh, how obviously very old it is, much older than the ninth century, and how clearly it has been handed on by bardic tradition even before the first traceable written texts, then one may accept without reservation Professor Williams's closing words, 'I think we can now begin to believe with a considerable degree of confidence that the body of the song is the authentic work of Aneirin'.

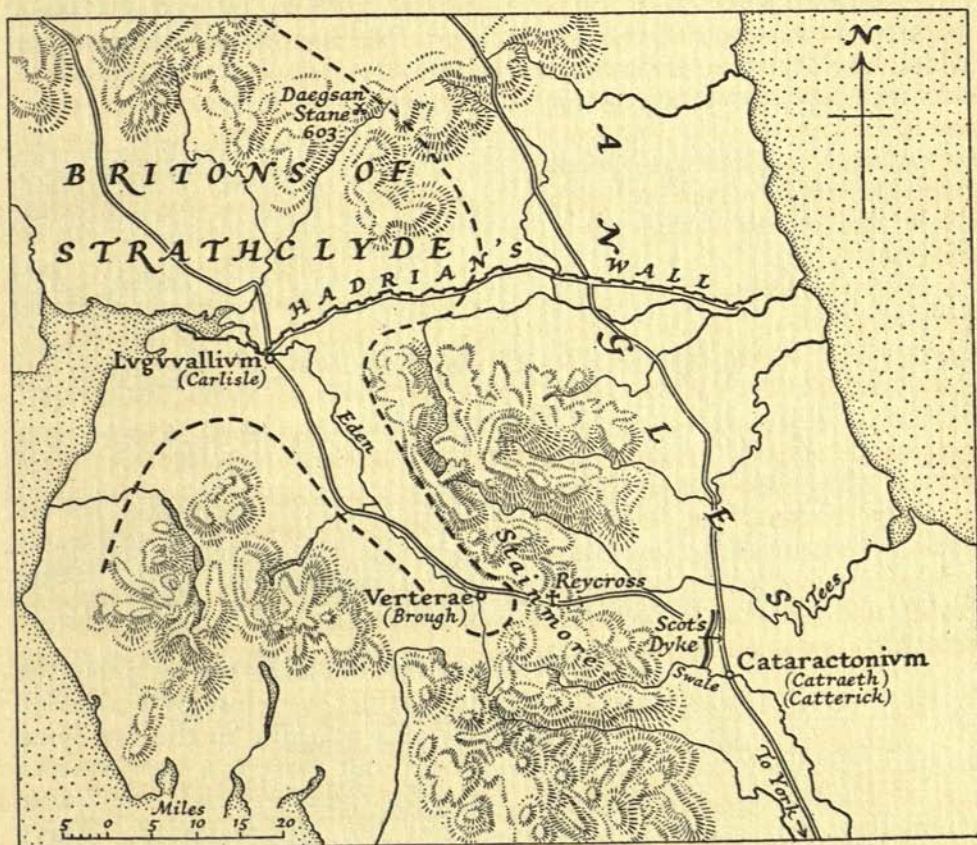
For over thirty years Professor Williams has been labouring at the difficult problems of the Book of Aneirin. Meantime, one work after another has established him as the greatest living authority on the language and literature of early Wales. One by one he has been unravelling the mysteries of the 'Four Ancient Books of Wales' and showing their true position and importance in the history of Dark Age Britain. The publication of *Canu Aneirin* is his *magnum opus*, though it is good to hear that he is now planning an edition of the Book of Taliesin. How all the more regrettable it is that he continues to write in Welsh works which would be of the highest value to hundreds of serious scholars and historians who cannot reasonably be expected to learn it to understand them.

NOTE ON CATRAETH AND CATTERICK

Catraeth was, however, within easy reach of the Eden valley—that wide corridor which pushes southwards right into the heart of the English uplands. That the valley was once part of the British kingdom of Strathclyde is to be inferred from the fact that in later medieval times the Rere Cross of Stainmore (which still stands there) was a recognized bound-mark between Scottish and English claims, and has been recorded as the southern limit of Scottish raiders (Skene, *Chronicles of the Picts and Scots*, 1867, 10, 204). From the head of the Eden valley a Roman road ran eastwards over the watershed from Brough past Rere Cross to join the main Roman road to Scotland 5 miles north of Catterick. From Brough at the head of Eden to Catterick is only 32 miles. A British force attacking the Angles of

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Northumbria could therefore safely assemble at or near Brough, and be within striking distance of Catraeth. From here Northumbria could be taken in the rear; by such strategy a small force would have much better chance of success than by a long and difficult southward march through hostile country along the east coast.



This view receives support from the course followed by the Scots' Dyke—a linear earthwork which faces eastwards (ditch on east) and extends from the Swale at Richmond, through Gilling to a point south of the Tees between Stanwick and Aldbrough, where it ends or is lost. The Scots' Dyke may well have been a defensive frontier marking the extreme eastern limit of British power (actual or claimed); it straddles the Roman road from the Eden valley just before this joins the Roman road from York to the north. The Roman Rig further south may have

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performed a similar function for the British kingdom of Elmete. (See Ordnance Survey Maps of Roman Britain and of Britain in the Dark Ages, South Sheet, and the accompanying text; also, for the Scots' Dyke, ANTIQUITY 1935, IX, 282 and plate opp. p. 277).

While this article was in the press a valuable note on the name 'Catterick' was received in a letter from Professor Ifor Williams, upon which the following remarks are based.

There are three places called Catterick :—

(1) The modern village in the North Riding of Yorkshire (Yorks, 54 NE), situated on the Swale, some $3\frac{1}{2}$ miles southeast of Richmond. $1\frac{1}{4}$ miles to the northwest of this village is the farm of Thornbrough, occupying the site of the Roman fort of Cataractonium.

(2) Catrigg, which is the name applied to a small stream called Catrigg Beck; to a waterfall on the stream called Catrigg Force, and to the pastures which form part of the gathering ground of this stream. These places are within the parish of Stainforth, north of Settle, in the West Riding of Yorkshire (Yorks 114 SW, SE).

(3) The Moor and Moss of Catterick, south of Stanhope and in that Parish (Durham, 32 NW).

Professor Ifor Williams writes that the best derivation for all these three is from the Latin word 'Catar(r)acta', which admirably suits two out of the three places. The Scandinavian 'force' means in fact a waterfall, so that in this instance there is a duplication of meaning.

With regard to the Swale, Professor Ifor Williams suggests that the Swale was called the Cataract river and that the fort close by was called after the river. There is no possibility of a waterfall in the low-lying ground near Catterick or the Roman fort; but there is a very obvious one at Richmond, and it seems probable, therefore, that the Swale might have been called the Cataract river after this waterfall. There is a further difficulty about the old forms of the name, that it seems to occur in two declensions—Caturacto, and Caturactonium or Cataractonium. Is it possible that the name was originally applied to a native hill-fort situated on the site of the medieval castle of Richmond? Is it not also possible that, if this were so, the battle of Catraeth may have occurred not at the Roman fort, which one would imagine had become derelict by that time, but rather on the heights of Richmond, close to the point where the Scots' Dyke ends.

O.G.S.C.

Origins of Plank-built Boats*

by JAMES HORNELL

IN works on the evolution of plank-built boats attention has never been directed sufficiently to the technique of the methods followed in their construction. The universal habit has been to take the vessels as they are in their completed state and to apply morphological methods to their study from this limited aspect ; precisely as the old-fashioned conchologist studied and classified his shells by outward form without regard to the soft parts—the essential criteria, as we now know. It is true nevertheless that in respect both of mollusca and boats, the study of the outward form has led, generally, to an approximation to the truth, but conclusions so reached are not to be accepted as correct without correlated verification dependent upon study of the kernel within the husk. Thus it is that nearly the same conclusions to which I come in this paper, have already been suggested tentatively by Brindley ;¹ but, owing to the fact that he did not utilize the internal evidence now brought forward, his suggestions were controversial hypotheses which he could not prove conclusively ; nor was he able to state definitely whether the divergent forms noted were reached by means of diffusion and local variation, or whether they arose through independent invention, or by a combination of the two factors.

Working on the hypothesis which I now put forward—that the only sound procedure in this study is to take into primary consideration the technique of construction employed by the builders of the various types of planked boats known to us—I come to the conclusion that there are at least four types which have had independent origin and evolution. These are :—

- A, the clinker or clench-built type ; characterized by inserted frames and overlapped hull planking
- B, the carvel-built type, planked on a pre-formed framework, with the planks meeting edge to edge
- C, the frameless river craft of Ancient Egypt and the present-day *naggr* of Nubia and the Sudan
- D, the junks of China, strengthened by bulkheads, in place of frames.

There can be no question that both clinker-built and carvel-built boats derive from dug-out canoes which have had their sides raised

* A paper read before Section H of the British Association, Cambridge, 1938.

¹ H. H. Brindley, 'Primitive Craft—Evolution or Diffusion', *Mariner's Mirror*, 1932, vol. XVIII.

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by means of planks. This conclusion is based upon the fact that the process is to be seen in operation in various parts of the world at the present day, and that it has been vouched for elsewhere by reliable observers within the past two centuries. These include India (Ratnagiri, Malabar, Tinnevely), Ceylon, Malay Archipelago, Solomon Islands and many island groups in Polynesia and others in Micronesia.² Africa provides several examples, notably the Baganda canoes of Lake Victoria, while North America provides an outstanding example in the log canoes of Chesapeake Bay.

Among these we find every stage from large and roomy dug-outs with sides raised by means of a single plank attached to the gunwales of the dug-out usually by so-called 'sewing' (PLATE I), up to those big vessels still existing in the island of Vahitahi in the Tuamotus which are constructed of numerous strakes raised upon the lateral margins of a channelled beam—the vestigial remnant of a dug-out base in which the hold has shrunk to a mere channel, a feature retained through that conservatism so frequent in the material culture of all countries.³ Between these extremes every stage can still be seen in existing examples.

While it is certain that all these vessels, whether clinker- or carvel-built, are derived ultimately from the simple dug-out, it will be found on enquiry that these two classes differ essentially in the methods by which the hull-planking is stabilized and formed into a structure capable of resisting the severe strains experienced during rough weather at sea. These differences in construction refer primarily to the way the planking of the hull is put together plank by plank, and in the second place to the means adopted to support the completed planking, the skin of the boat, in correct shape and so to prevent deformation and the opening of the seams.

CLINKER- OR CLENCH-BUILT BOATS

In the case of clinker-built boats the hull planking is assembled by overlapping each successive strake or run of planking on the outer and upper edge of each lower strake; in primitive types this overlap seam is secured by one of several methods of lashing or 'sewing', by means of cord or thong passed through holes in the edges of the planks, a method replaced by the use of fastenings of metal when knowledge of working iron was attained, coupled with a supply obtainable in adequate quantity.

² A. C. Haddon and J. Hornell, 'Canoes of Polynesia, Fiji and Micronesia: *Canoes of Oceania*', B. P. Bishop Museum, special publication, no. 27, 1936.

³ J. Hornell, 'The Evolution of the Clinker-built Lugger', *ANTIQUITY*, 1936, x, 341-5.

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So long as the dug-out remains the major element in a clinker-built boat and the sides consist of only a few strakes, the strength and structural unity of the dug-out region are sufficient to withstand the stresses encountered at sea, more particularly as a boat of this description is so narrow in the beam that the weight carried is very limited and is distributed fairly evenly throughout its length.

To provide increase in carrying capacity the depth of the vessel has to be increased by the attachment of additional strakes. In practice it is found at once that this entails a loss of stability if the planks are placed vertically; they must flare outwards and thereby increase the beam if loss of stability is to be avoided. But if the planks be placed at a considerable angle from the vertical the lowest or garboard strake will make a weak joint with the gunwale of the dug-out. To meet this difficulty the sides of the dug-out are cut away or cut down drastically and so fashioned that the garboard strake may be attached more easily and more securely. Even so, the joint is insecure and becomes weaker with every increase in the width and flatness of the bottom, which, it may be mentioned, depends upon the angle subtended by the garboard strake with the dug-out base. This base henceforward becomes narrow and is in transformation into a true keel—the backbone, so to say, of the vessel.

To overcome the defect thus developed came the invention of inserted frames or ribs, that is, of stout bars of curved timber inserted or placed transversely within the hull at regular intervals throughout its length *subsequent* to the fitting together of the hull planking. In northern Europe the clinker build has always been the distinctive type of boat construction. There, from the end of the Bronze Age until the end of the true Viking Age (c. A.D. 1000), these inserted frames were attached to the skin planking by the intermediary of transversely placed series of comb-cleats, or perforated 'lugs' left upstanding when the planks were adzed into form. Through the perforations in these cleats lashings were passed, securing the inserted frames to the planking. In the earliest known clinker-built boat, that of Hirschsprung in Als (c. 500–400 B.C.)⁴ the strakes are few and exceptionally broad, with the cleats in sets of four or five on each plank. With advance in constructive skill, the planks became narrower and the cleats fewer, till at the culmination of the Viking Age in the 8th and 9th centuries A.D., they became so narrow as to admit of only one cleat on each plank in each

⁴J. Hornell, *ANTIQUITY*, 1936, x, 342.

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series. This distinctive narrowness of the planking in the medieval and modern clinker type is associated with notable thinness, seldom exceeding threequarters of an inch, thereby contrasting markedly with the broad, thick planking of carvel-built boats. In the clinker type there is no need to caulk the seams; in the carvel this is an absolute necessity. The thin and narrow character of the planks used in the clinker build is of great advantage to the shipwright for it enables him to bend his planks easily. In early days when facilities for steaming and bending planks were lacking it enabled the northern clinker builder to evolve a type of boat of finer lines and greater beauty of curve than was usual in the southern region (Mediterranean) where the carvel build alone was in use.

With the replacement in northern lands of the adze by the saw in the production of planks for boat-building purposes, which I deduce occurred about the end of the 10th century, the use of cleats and lashed-on frames was abandoned in favour of riveting the planks directly to the frames. This, however, did not bring about any change in the custom of inserting the frames after the fitting together of the skin planking; even to the present day this sequence persists. As will now be shown this characteristic remains one of the distinctive differences between the clinker and carvel builds.

The clinker build is distinctively northern in its distribution. It was in use at the overlap of the Bronze and Iron Ages in Scandinavia and the only build employed by the Vikings. It remains the characteristic system of construction of small craft in Scandinavia at the present day and this is true equally of those fishing ports in Great Britain where old types survive. It is or was the system favoured in the building of Orkney and Shetland boats, the old Scots' 'Fifies', Humber and other East Coast keels, Tyne wooden tugs, Fenland barges, Yarmouth beach yawls and luggers, Deal galleys, Hastings luggers, East Coast billyboys and many other local types, at those ports where the smaller fishing craft are found. With the great increase in size that has marked fishery development during the past half-century the impulse has been strong to change to the carvel build as being the stronger in framework and in the thicker planking used, necessities that arise when longer voyages, more powerful fishing gear and greatly increased carrying capacity are required. The tendency of the deep-sea fishing industry to pass out of the hands of individual fishermen into those of capitalistic companies operating a fleet of vessels has been an important factor in the change, for the carvel type is more expensive

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to build than the clinker. The fisherman-owner has small capital and seldom can afford the heavy cost of the large craft that are now requisite, built either with the carvel type of wooden hull or with steel, if profits are to be made and competition met. Another reason given by fishermen for the abandonment of the clinker build is that the heavier carvel is better adapted to withstand damage during the rough and tumble struggle for position in a congested port during the height of the herring fishery. It is economically impossible to build clinker boats with thick and heavy planking.

THE CARVEL BUILD

Today this is the distinctive type of craft plying in the Mediterranean and Indian Ocean areas. There are no exceptions and so far as we can judge from all the evidence available, it was equally the distinctive and sole type of boat-building in Greek and Roman times and was also characteristic of Phoenician shipping and of early Persian and Arab vessels. (PLATE II).

The features that differentiate this from the clinker build are that (a) the frames or ribs are pre-formed and are erected upon a keel in their permanent position before the planking up of the bottom and sides ; (b) when the planking is in place, each strake is nailed, spiked or bolted to the frames already fixed in position ; (c) the planks in each strake are fitted edge to edge to those on either side ; (d) some soft material (oakum, cotton, rags, moss or fibre) is requisite to caulk the seams between the various strakes, and (e) a greater thickness of planking is needed in order to allow of efficient caulking.

No direct bond between the planks of one strake with another is present apart from one exceptional instance (Gujarat boats),⁵ cohesion being effected indirectly by spiking or bolting the planks to the rigid frame-timbers, reinforced by the interstitial caulking.

The power to employ thick and stout planking which is characteristic of this system of construction has enabled the shipwright to build larger and stronger vessels than is possible with the clinker system. Hence, even in northern Europe, the home of the clinker system, the only boats now built in this manner are fishing and pleasure craft of limited size and tonnage. For inshore fishing the clinker build holds its own still in that area, for it is cheaper to build and lighter to handle than a carvel boat of similar dimensions.

⁵ J. Hornell, 'The Tongue and Groove Seam of Gujarati Boatbuilders', *Mariner's Mirror*, 1930, vol. xvi.

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The light shed upon the processes whereby dug-outs have evolved into planked boats, which is afforded by the history of the Chesapeake Bay log-canoe already alluded to, is of exceptional value and deserves to be summarized here. As is detailed in a handsome monograph published by the Mariners' Museum, Newport News, Virginia, the English settlers on the shores of Chesapeake Bay gradually evolved from the crude, square-ended dug-out used by the Indian tribes with whom they made contact, an exceptional type of craft capable of carrying a great spread of canvas and with a hull exceedingly fine-lined and handsome. Although this type of craft is doomed to early extinction due to the advent of the cheaply built motor boat, a few still exist as racing craft, built in the traditional fashion. These depend for their strength upon a backbone formed of a dug-out, with its sides heightened (typically) by means of two wide log-strakes set carvel fashion on either side, to which are now added a number of refinements. These include stem- and stern-posts, one or two upper strakes of planking, supported on *short* stanchion-ribs which have developed from knees ; there is also a wide washboard inclined at a slight angle from the horizontal, to form a narrow deck along each side. The larger are fitted with two masts and a bowsprit, and carry when racing a grand spread of sail including fore-sail, mainsail, topsails, jib, staysails and sometimes even a spinnaker. To give stability a powerful centreboard was added, seeing that none of these craft, no matter how improved, ever had a keel fitted to the basal dug-out of the hull.

Just as the clinker and the carvel builds have diverged in their constructional methods from the common parentage of the dug-out canoe, to converge ultimately in forms closely akin in outward appearance and in performance, so from the primitive raft, an even more elementary invention for water-transport, two distinct types of plank-built craft have arisen—possibly even others. Of the two with which I shall deal, one type is that of the Ancient Egyptian river and coastal boats, the other the junk type of the Chinese area.

ANCIENT EGYPTIAN BOATS

The ancient boats of Egypt were essentially vessels of light draft intended for traffic on the river Nile ; probably they were used occasionally for expeditions along the coasts of the Red Sea in the fine weather season. They were beamy boats, without trace of keel, shallow and round-bottomed. Neither had they any frames ; there were neither

PLATE I

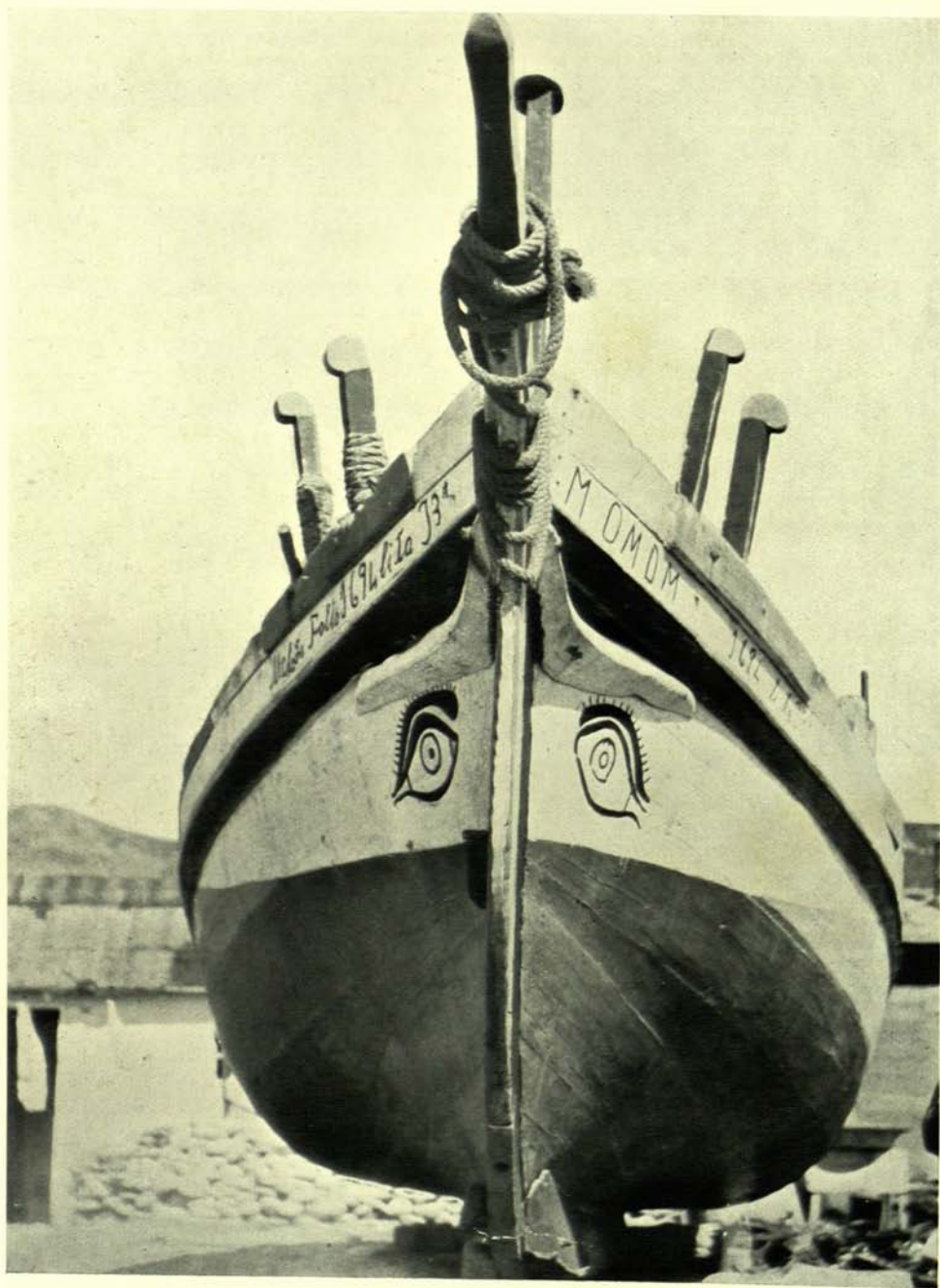


A MASULA BOAT, MADRAS, SHOWING HOW THE PLANKS OF EARLY CARVEL-BUILT BOATS
WERE SEWN, EDGE TO EDGE

The daubed pot on the prow is to avert the evil-eye and propitiate the local goddess

Ph. J. Hornell

PLATE II



A MALAGA FISHING BOAT OF GALLEY TYPE, CARVEL-BUILT OF STRAKES NAILED TO FRAMES
Painted oculi on the bows

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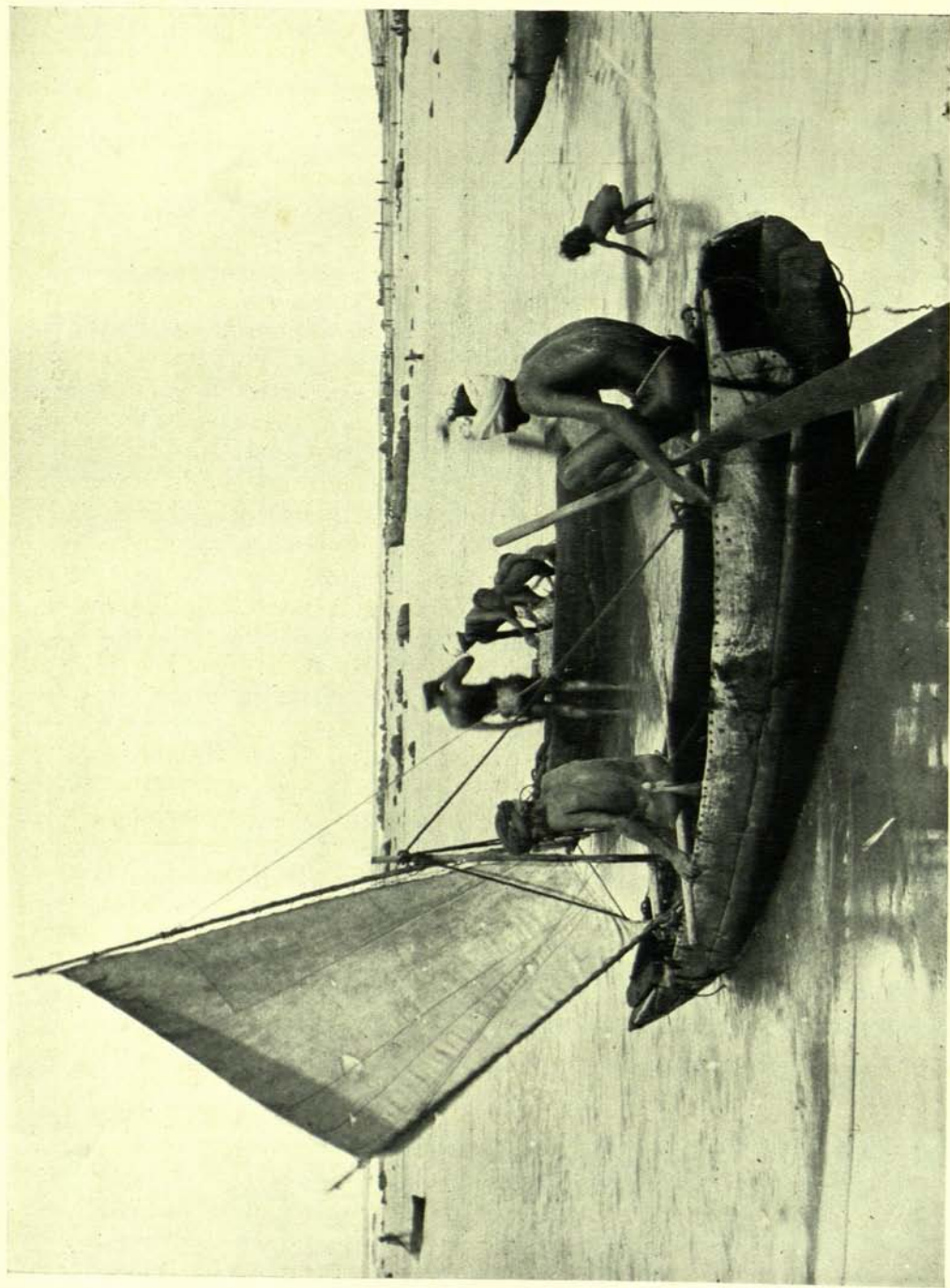
PLATE III



A SPECIALIZED CATAMARAN, VIZAGAPATAM, EAST COAST OF INDIA, TAKEN APART TO DRY
The outer log on each side is pegged on, whereas the side planks are tied on by widely separated 'stitches'

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PLATE IV



A VIZAGAPATAM CATAMARAN IN THE ASSEMBLED CONDITION, RIGGED TO SAIL

The parts are bound together by circumferential lashings at head and stern

Ph. J. Hornell

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floor timbers to knit the bottom planking together, nor anything in the nature of ribs to give support to the side strakes. Outwardly they were similar in appearance to the carvel build, for the planking was laid edge to edge throughout. As no framework was present either inserted or pre-formed, the planking was held together partly by broad dowels inset in the edges of opposed planks, partly by double dovetailed tenons inset on the inner side linking the butted ends of the planks in the various strakes, with the whole hull held together by numerous stout thwarts having their ends mortised into the sides below the gunwale.

I am strongly inclined to think that this method of boat construction was evolved by men who were by trade land carpenters and masons ; men whose trade was primarily to make household articles—boxes, furniture and the like—for this system of tenons, dovetails and struts is characteristic of the wood-working of Ancient Egypt and to a lesser extent of their stone-work as well. The resultant craft are just what we should expect to be evolved from attempts to translate the form of the papyrus rafts so common in Early Egyptian paintings and gravings into one of wood, built up of planks. As evidence of this origin we have the circumferential painted banding near the bow and stern in typical representations of these ancient boats, continuing to perpetuate the memory of the rope lashing around the ends of papyrus rafts or raft-canoes at the places where the bundle ends are bunched together to form the attenuate stem and stern respectively. This view is the one generally held so I need not elaborate it further.

The present-day craft of the Nile, with one notable exception, are of wholly different type. Like the boats of the Mediterranean, Red Sea and Indo-Pacific Ocean they are carvel boats, planked upon a pre-formed framework of rib timbers. So far as we know this change must have occurred in comparatively modern times, for the ancient river-types appear to have been little affected by Greek influence even under the Ptolemies, whatever may have happened in the case of sea-going vessels ; for this dynasty, foreign though it was in origin, identified itself so closely with the thought and religion of the land that old traditions continued unbroken. Whether Rome exercised any important influence we cannot say ; probably not, for she wisely left such things alone, content so long as Caesar received his due. However this may be, it seems probable that Mediterranean influence had been slowly creeping in and affecting many age-long customs from the early centuries of the Christian era. The final impetus was received when the Arab swarms overran the country in the middle of the 7th century. Until Muhammad's

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time the Arabs had remained a nomad desert people, without taste for seafaring. Once contact was established with Byzantium and Europe there came a quick and surprising change. The Arabs, nimble witted and eager to learn, engaged Persians and renegade European Christians as their teachers in sea-craft ; they proved fairly apt pupils. The hull construction which they adopted was that common in the Mediterranean at that period, the true carvel, built on a pre-formed framework, and it is this that we meet with exclusively in the Nile today until we reach Edfu in Upper Egypt. There we meet vessels which have unusually thick strakes pinned together by means of iron spikes passed obliquely from above downwards through the opposed edges of adjoining planks—a method based upon the dowel system of fastening used in Ancient Egypt. There are a few frames, usually not more than four or five, widely spaced and obviously put in as a borrowed afterthought, because they are inserted *after* the skin planking is put together as in the clinker-build. As evidence of insertion after planking, we find the nails or spikes driven through from the interior of the hull, and with their points clenched down on the outside. This fact proves that these inserted frames are not of northern origin, for there the planks are attached to the inserted frames by nails driven in from the exterior and clenched or riveted on the inside. To compensate for the poverty of framing, specially stout beams cross the hull, tying the sides together ; as in Ancient Egyptian boat construction, a feature of these boats is the penetration of these beams through the sides whereby their ends are visible and usually flush with the outer surface.

Farther up the Nile, between Wadi Halfa and Khartum, this old type, the *naggr*, approximates still more closely to the ancient type, for in this region the *naggr* has no framing whatever. It has simply a hull made of extra thick planking held together by oblique spiking through the seams and by numerous thwarts or beams across the hull, penetrating the side planking at deck level. This survival, partial in Nubia and complete in the northern Sudan, has been the fate of other ancient Egyptian customs and techniques which have been obliterated in Egypt proper through Arab and Moslem influence, but yet survive in the Sudan ; among them is the use of rude raft-canoes by the Shilluks and others, constructed of light sticks lashed together. These, however, are wedge-shaped and pointed at one end only, a difference either due to the use of different and less pliable material, or representing a survival of a primitive type upon which the Egyptians improved when they began to use papyrus instead of sticks for this purpose. Of still

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greater affinity are the bundle-canoes of Lakes Tsana and Chad ; here the original papyrus material continues to be employed with the stern tapered or sharp in the ancient fashion. Some are large enough to carry cattle.

THE JUNK DESIGN

The design of the Chinese junk is subject to great variation in these latter days, for builders frequently incorporate European characteristics in their modern designs. So we find some junks built with a keel and in others the planking rounds into a true stem-post.

The junk design exemplified in the oldest and least modified type has a carvel-built hull wanting in all the three constituents which are considered in Europe to be essential in the construction of a ship—keel, stem-post and stern-post. The bottom as in ancient Egyptian vessels is rounded transversely, and the planking, instead of closing in at the stern, ends abruptly, giving an open end which has to be closed in by straight planking, so forming a transom stern. Similarly, in the oldest of the junk and sampan design, the fore end is also truncate, closed by cross planking in transom fashion. The hull, reduced to its simplest expression, may be compared to half of a hollow cylinder bent upwards toward each end and there closed by a terminal partition. Frames are replaced by solid bulkheads of which the fore and the stern transom may be reckoned as the outer units. This use of bulkheads gives a hull fully as rigid and strong as one planked upon a pre-formed framework even though the bulkheads may be less numerous than the frames. Construction of this nature has the added important advantage of providing a number of water-tight compartments, thereby lessening the danger of sinking should the vessel spring a leak or be damaged below the water-line. In this as in several other nautical inventions the Chinese anticipated modern European usage by many centuries.

As I have shown elsewhere⁶ this junk design is derived from a raft-form such as is, or was till recently, in use on the coasts of Formosa. In this elaborate type of raft the platform is composed of lengths of long bamboos lashed in horizontal position, narrow ends forward, to curved poles laid transversely over them—the equivalents of the frames which we find in carvel and clinker builds. The platform bamboos sheer upwards toward each end and if these be replaced by planks and both the longitudinal and transverse curves be accentuated, we should obtain

⁶ *Mariner's Mirror*, XX, 331-7.

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a hull form approaching that of the junk. It would want only the transom closing of the semicircular end openings and the conversion of the pole frames into bulkheads to make the transition complete.

In this evolution we note that the early junk design would approximate to that of boats derived from dug-outs in the use of internal frames upon which the bulkhead system was introduced as an improvement.

Should these facts be considered inadequate to the conclusion that junks, like Ancient Egyptian river boats, are derived from the translation of a raft structure into a plank-built form, I would adduce contemporary evidence from India to show how an analogous change from a simple raft to a craft of incipient boat form is in course of actual evolution there. As is well known the distinctive fishing craft on the exposed and surf-swept eastern coast of India is the catamaran. Typically, as seen on the beach at Madras, it consists of a number of logs, usually three, sometimes five, and at Negapatam seasonally rising to seven, arranged and tied side by side. The number is typically uneven, for catamarans are of traditional build, sharply pointed at the fore end, wide and usually truncate in steps at the after end. In the Vizagapatam District, however, an advance has been made whereby a plank-strake has been pegged on at each side, rowing thwarts and sometimes a serviceable rudder installed, together with the provision of a mast and sail, regularly used, whereas the more primitive type seen at Madras never sets any kind of sail. (PLATES III, IV). These Vizagapatam catamarans have the after-end open; were such craft to be in use on any sea except one where the rigours of a northern winter are never experienced, its open seams would be made watertight by caulking and the after-end closed in by planking as in the Chinese junk.

The evidence brought forward above is sufficient, I believe, to substantiate the view that of the four types of plank-built boats that have been dealt with, the two in present-day use in Europe have evolved from dug-out canoes, identical in form and origin. It is equally clear that they evolved by different methods into forms superficially remarkably similar and with the capacity to perform like duties.

The other two types are just as clearly derived through two distinct lines of evolution from raft forms of different shape and material; the Egyptian from the two-ended and pointed papyrus raft-canoes, the Chinese from one constructed of bamboos or of logs of other species of timber.

The Plough and the Origin of Strip-Lynchets

by E. CECIL CURWEN

IN their recent work, *The Open Fields*,¹ C. S. and C. S. Orwin devote an appendix to the question of the origin of lynchets, in which they express their difficulties in accepting the current theory that narrow, step-like terraces like those at Bishopstone (Wilts.), for instance, could have been formed as a result of ploughing (PLATE I, facing p. 49). In private correspondence they assure me that these difficulties are widely shared by other agriculturalists, but at the same time they admit that they have no better explanation to offer. They point out that these terraces are found distributed mainly on the chalk of southern England, but also on various other geological formations in the Midlands, west and north, and also in southern Scotland²; also that there seem to be many varieties of strip-lynchets, and that possibly one explanation may not apply to all; but while rightly distinguishing between these and the small square plots of the Celtic field-system, they are very much in error in implying that the latter are bounded only by field-banks and not by lynchets (p. 319).

The authors state the case for the plough-theory in the chalk areas as follows:

'The suggestion is that when the strips of the ridge and furrow reached the hill-side, the bank above them was then laid out for ploughing in strips, with grass balks between each, but that the ploughmen, ploughing along the hill-side, found that their ploughs would not turn furrows uphill. And so the plough was dragged back idle, to plough a second furrow downhill against the first, and so on until the whole strip had been ploughed in furrows lying along the slope, downhill. Repeated ploughings would build up the soil at the bottom of the strip and dig out a bank at the top of it' (pp. 320-1).

They then proceed to enumerate five objections to this view, and these may be summarized as follows:—

¹ Oxford University Press, 1938, 21s.

² Cf. *ANTIQUITY*, 1932, VI, 402-4.

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(1) Some lynchets occur in places where topography makes it unlikely, if not inconceivable, that they could have had any connexion with an open field.

(2) Take the case of a lynchet 20 ft. high in the vertical, and consisting of positive and negative elements superimposed, 'it means that the slope of the hill has been dug out by ploughing to a depth of some 10 ft. . . . Is it conceivable that the ploughs of the Dark Ages could cut into this chalk rock to a depth of some 9 ft. and turn it downhill to form a flat terrace'? And if they could, what becomes of the fertile top-soil in which to grow the crops?

(3) What happened after the terrace had become level? Did one-way ploughing continue, so as to pile up soil at the outer edge? or did the ploughman revert to the normal practice of ploughing up high-backed ridges? Neither of these appearances is normally found on these lynchets. What therefore did he do?

(4) Strip-lynchets do not always approximate in their plan to the standard English acre, 220 yds. by 22 yds., or its fractions.

(5) If lynchets are to form between adjacent strip-cultivations a balk of unploughed turf must have been left to prevent the soil from the upper strip from travelling down to the lower. But the authors have shown earlier in their work that such balks did not normally exist in the open-field system (*pace* Seeböhm), therefore lynchets could not have been formed in the way suggested.

I am unaware of any satisfactory sections having been cut through strip-lynchets, and this is obviously the first essential to a proper study of the subject; but having had considerable experience in planning and sectioning the lynchets of the Celtic field-system, I welcome this clear-cut statement of the case against the plough-theory because I feel that the specific difficulties raised are not really valid objections in view of the available comparative evidence. At the same time I feel that the evidence that we have necessitates the plough-theory as the only possible explanation, though I am willing to admit that further research might reveal types of strip-lynchet for which some other explanation might be required, *e.g.* possible vineyard-terraces. (Where were the vineyards that the Normans cultivated?)

The two most formidable arguments against the plough-theory are obviously the second and fifth. Before considering them it will be well to review the other three, viz. Nos. 1, 3 and 4 on the above list.

Nos. 1 and 4 go together, for they turn largely on the question as to whether all strip-lynchets must belong to the open-field system. That

THE PLOUGH AND THE ORIGIN OF STRIP-LYNCHETS

some examples do so belong seems an inescapable conclusion when (for instance) the plans of those at Calstone Fields (Wilts.)³ and Worth Matravers (Dorset)⁴ are examined. There the general lay-out and dimensions of the strips correspond with those of the extant plan of the local open-field in the first case, and a typical plan of part of an open-field in the second. But I am quite open to conviction that some other examples of strip lynchets may not belong to an actual open-field, but yet may none the less have been the result of ploughing. Under such heading might come the lynchets of the Scottish Border districts.

Objection No. 3 presupposes that high-backed ridges were ploughed up on such dry soils as the chalk—for the lynchet-question concerns the chalk areas more than any other—and the same implication is made in the authors' statement of the plough-theory, already quoted. But they have shown in their book that ridge and furrow ploughing was done on soils that required drainage, the furrows always running at right angles to the contours for this purpose. This resulted in high-backed ridges or 'lands' such as we see in the Midlands today; but on dry soils the position of the furrows was changed annually in order to prevent the formation of ridges. It is thus particularly on dry soils that the plough could afford to travel *along* the contours, instead of across them. When the slope was steep enough to necessitate the sods being turned downhill every time, this did not necessarily mean that the plough had to return idle to the starting-point. In old wooden Sussex ploughs the mould-board could be quickly detached and fixed on the other side, with a corresponding adjustment of the coulter, so as to turn the sods the opposite way. If this adjustment was made at the end of each furrow, the sods could be turned downhill on each journey, irrespective of the direction. If this was not the intention, what was the purpose of this provision on the plough? When a lynchet-terrace had become more or less level as a result of long cultivation, turning the sods downhill year by year, it could then be kept level by turning all the sods in opposite directions in alternate years; *i.e.* all uphill one year, and all downhill the next. High-backed ridges were in any case not used on the chalk, so that this objection (No. 3), it seems, has little point.

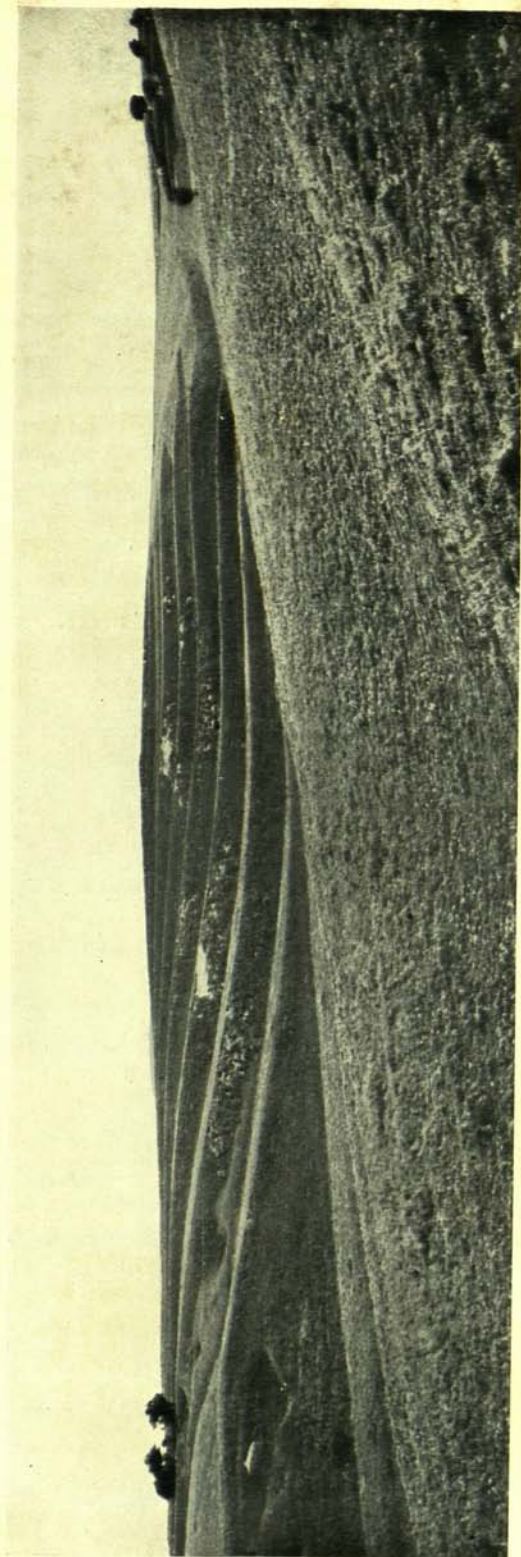
Objection No. 2. The authors question the possibility of a light plough biting 9 or 10 ft. into solid chalk along the upper edge of the

³ Crawford and Keiller, *Wessex from the Air* (1928), pp. 166-8 and pls. xxviii, xxix.

⁴ *ANTIQUITY*, 1927, I, pl. III, f. p. 272 (air-photo).

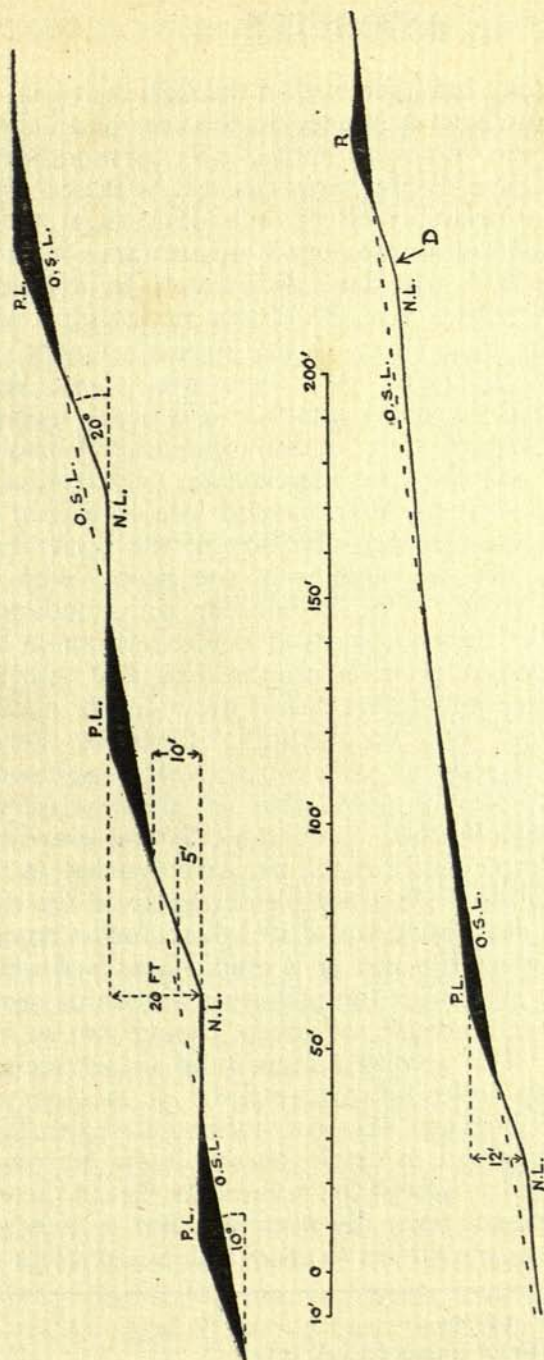
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strip, with resulting infertility of the bare chalk thus exposed; this is, given a lynchet face with a vertical height of 20 ft. (see page 49), not just measured up the slope. If we take the original slope of the hill as 10° , and the slope of the lynchet-face as 20° , its *vertical* height being 20 ft., then the depth of the excavation along the upper edge of the strip will not be fairly represented as 10 ft. or even 9 ft., but it must be measured in a direction perpendicular to the *original* slope of the hill, and will be found to be about 5 ft. when drawn to scale. It may still be said that the ancient light plough could not do this, but the Celtic field-system provides plenty of evidence that it could and did do so. In *Antiquaries Journal*, XIII (1933), plate XVIII (f.p. 120) may be seen a scale-drawn section made through such a field on Thundersbarrow Hill (Sussex), cultivated between the 1st and 4th centuries A.D. The plan of these fields is given on plate XVI (f.p. 116), where the position of the section is not indicated, but it cuts through the field immediately to the left (west) of the pre-existing camp (5th cent. B.C.), and slightly to the north of 'platform 4'. Nearly all these fields are outlined by massive lynchets, and there are also traces of strip-cultivation overlying the Romano-British fields on the east side of the camp. In the section (pl. XVIII) the field in question was about 195 ft. wide when laid out on the original hill-surface, and the latter sloped down from the rampart of the old derelict 'camp' (seen on the right) at an angle of about 7° . At the lower edge of the field there is now a massive lynchet measuring nearly 12 ft. in vertical height, and face sloping at a little over 20° ; a trial hole dug here showed that this consists, as one would expect, of a positive on top of a negative element, in nearly equal proportions. At the upper edge of the field stands the rampart of the old camp, and underneath this was found the old turf line marking the surface of the hill about 500 B.C. before the camp was constructed. This turf-line ends abruptly at the point where the upper edge of the field started, and here was found a deep negative lynchet, similar to that at the top of the next field below; its *vertical* depth below the edge of the old turf-line underlying the ramp proved to be 6 ft., but measured perpendicularly to the original surface line the chalk had been bitten into to a depth of only about 3 ft. The position of the original surface-line was determined by digging trial holes at intervals, and also by the old turf-line already referred to. It is evident from the section (see page 49) that all the top-soil and a large amount of solid chalk have been removed from the upper half of the field and spread over the lower half, to a maximum depth of 3 ft. at the crest of the lynchet at



STRIP LYNCHETS AT BISHOPSTONE, WILTS (see p. 45)

Ph. Mrs C. S. Orwin



Above: HYPOTHETICAL SECTION THROUGH A SERIES OF STRIP-LYNCHETS, EACH 20 FT. HIGH, ON A HILL-SIDE SLOPING AT 10 DEGREES
Below: ACTUAL SECTION THROUGH A FIELD OF THE ROMAN PERIOD ON THUNDERSBARROW HILL, SUSSEX, SHOWING THE ORIGINAL SURFACE LINE, POSITIVE AND NEGATIVE LYNCHETS, AND RAMPART OF SMALL IRON AGE CAMP

PL., positive lynchet.
 N.L., negative lynchet.
 O.S.L., original surface line of hill before cultivation.
 R, rampart of Iron Age Camp.
 D, position of ditch of Iron Age Camp, destroyed by negative lynchet.

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its foot. The same thing has happened in the field next below, and the plan shows that it must have happened in the case of all the fields on that hill. In fact, in the downland immediately surrounding Brighton the lynchets of the 'Celtic' field-system can still be traced over an area of about 9000 acres, associated with 32 occupation-sites of the Roman period, and all contained in an area of 65 square miles.⁵ It would be absurd to suggest that this was done deliberately by digging, for the section just described shows that the degree of levelling obtained is negligible, and is more than offset by the impoverishment of the soil in the upper half of the field. Moreover who would deliberately transport so much chalk by hand across a field nearly 200 ft. wide? The conclusion that this soil-creep is the unintentional result of long-continued ploughing seems to be inescapable. If then, the simple plough of the Romano-British peasant could bite so deeply into solid chalk, even at the expense of the infertility of the upper half of the field, then why could not the ploughs of the people who made the strip-lynchets do the same? One is familiar with moderately deep negative lynchets at the upper margin of modern fields on the chalk, and it is very noticeable how poor the crop is along that margin for that very reason; but I have noted that it *does* grow on the chalk without any appreciable mould, but very thinly. Professor Percival has pointed out how ancient peasants were content with much poorer crops than we are.⁶ It may also be noted that (a) there is never an old turf-line under a positive lynchet, comparable to that found under the rampart of the old camp; and (b) the negative lynchet at the upper edge of the field in the section has completely removed the outer ditch of the camp; (c) the downward soil-drift which forms lynchets does not necessarily presuppose the use of a mould-board plough turning the sods downhill, for even with the primitive scratching plough on a slope, rainwash is bound to cause soil-creep, though not so rapid.

Objection No. 5. The second argument of importance concerns the question of grassy balks between strips. It is obvious that if lynchets were to form between adjacent horizontal strips, balks must have been left between them to catch the soil in its downward drift. We may also be prepared to accept the arguments that in the open-field system such balks were not normally present. But it does not follow that if the medieval peasants saw fit to allow individual strips to terrace

⁵ ANTIQUITY, 1935, IX, 443-54.

⁶ J. Percival, *Wheat in Great Britain* (1934), 39-43.

THE PLOUGH AND THE ORIGIN OF STRIP-LYNCHETS

themselves on the hill-side—as they quite obviously did at Calstone Fields—they did not purposely leave balks for that very purpose. Present-day practice on the Continent supports this view. In *ANTIQUITY*, 1932, VI, pl. IV (f.p. 393) may be seen a photograph of narrow strip-lynchets actually in process of formation by ploughing in Belgium, with grassy balks, gradually becoming the faces of lynchets, very much in evidence. In the same country I have noticed that on more level ground the cultivated strips are not separated by balks. The French burn the grass on the faces of the lynchets to prevent it from seeding, and such local customs as 'la jambe pendante' betray a full consciousness of the fact that their lynchets have been built up by ploughing, and that each consists of a negative and a positive element (French *entaille* and *remblai*, respectively).⁷

It is probable that the study of present-day agriculture in some parts of the Continent where lynchets are still allowed to form may help to answer most of the other objections. One cannot escape the conclusion that as the plough must have formed the massive lynchets of the Celtic field-system, and does sometimes produce lynchets on the Continent still, there is no reason why it could not have produced the strip-lynchets in question. It is conceivable that some series of hill-side terraces might have had a different origin, but I know of no factors as yet to render this probable, and as the plough-theory must be correct for some, and there is no rival theory for others, I feel that it is not unscientific to let the plough-theory stand for the present.

The isolated sets of terraces that do not look like outlying parts of an open field (e.g., in the Scottish Border districts) might, to my thinking, have been deliberately formed on steep slopes facing the sun in order to counteract the effects of the northerly latitude in the effort to grow wheat. The surprising thing is, not that such terraces should have been formed by the plough, but that they should have been formed at all, if more level ground was available.

The question as to whether a given series of lynchets has been formed incidentally by ploughing, or deliberately by digging, depends to a large extent upon whether the original slope of the hill was too steep to be ploughed without previous deliberate terracing. This factor is difficult to estimate, but lest any should consider a slope of 20° too steep for ploughing parallel to the contours, one may cite the case of a very large positive lynchet at Asham, near Lewes, showing on section the cardinal signs of having been formed by ploughing and not by

⁷ *ANTIQUITY*, 1932, VI, 400.

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digging (see below). Here the slope of the original hill-surface before the lynchets was formed was as much as 20° in the lower part of the field, diminishing in steepness towards the brow of the hill.⁸

When, as one hopes, sections come to be dug through a series of strip-lynchets on a steep hill-side, there are two criteria which should afford positive evidence (on chalk subsoil) as to whether the terracing is the result of ploughing or deliberate digging.

(1) There will be no old turf-line beneath a positive lynchets if it results from ploughing, as the plough breaks up the original turf and mixes it with the chalk that is washed down from above. If formed by digging, the chalk is deposited on an undisturbed turf-line.

(2) If formed by ploughing, a positive lynchets will consist of fine chalk detritus—powdered by the action of frost and slowly deposited by rain-wash. If formed by digging it will consist of chalk rubble similar to that found in the ramparts of hill forts.⁹

⁸ *Sussex Arch. Coll.*, 1930, LXXI, 254.

⁹ Both these conditions are fulfilled by the Shawford lynchets, which are exposed by a longitudinal section in a railway-cutting south of Shawford station, between Winchester and Southampton. This section was cut back some years ago, and I have seen it many times. There is no trace of an old turf-line anywhere, and the material of the lynchets is a fine chalk detritus. I have seen other lynchets-sections elsewhere which agree with this.—O.G.S.C.

Early Scotland

MAP OF BRITAIN IN THE DARK AGES, North Sheet. Scale 1:1,000,000 [16 miles to 1 inch]. *Ordnance Survey Office, Southampton, 1938.* 5s. (The map only, without text or cover, 2s 6d).

THIS map', the Director General informs us in his Foreword, 'forms the northern portion of the Map of Britain in the Dark Ages, of which the southern portion was published in 1935'. It represents therefore the completion of a task which must have taken nearly a decade to carry out, from its first inception down to publication. This northern sheet is in fact a map of Early Scotland, showing it as it was roughly between the years A.D. 450 and 850; but in this dim historical dawn exact dates cannot as a rule be discerned. A long historical introduction of 26 pages, and 5 diagrams, serve to amplify and elucidate the map itself; and there is of course the usual index of place-names.

First impressions are important and often more trustworthy than second ones. Our impressions on opening the map for the first time was one of surprise that so many names and sites should have been available for a period and region that is so poorly documented. At the same time one was forcibly struck by the real beauty of the map which, we think, must represent the highwater of technical skill and craftsmanship in lettering, in symbols and in printing. The lettering, like that of the southern sheet, is the one adopted for the current Ordnance Survey one-inch map, and it is eminently suitable, for no more beautiful alphabet exists. The larger names are written in such a way as to indicate so far as possible the areas, always vaguely delimited at that date, to which they refer. They are consequently not written haphazard, but often follow graceful curves whose execution must have demanded considerable skill and judgment. It must have been difficult for instance to show in a map such as this the exact course of Drumalban (here spelt, with almost pedantic exactness *Druim nAlpand*). The Backbone of Britain was actually the mountain zone that separates the East of Scotland from the West; but being, geologically, a much-dissected peneplain, and being also breached not only by the Great

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Glen but also by innumerable other valleys and low passes, it has no appearance of uniformity on a layered map. Nevertheless, for all its breaches, it was a well-recognized physical barrier in ancient times.

The symbols are always of importance on a map which is intended to be read carefully, and to convey a message by visual graphic methods. The table of reference, if carefully examined, reveals a systematic scheme carried over from the Southern Sheet, with the necessary modifications. There were no towns in Northern Britain at this date (though we think a case might be made out for Carlisle, *Lugubalia Civitas*). 'Other places' are mostly derived from historical sources which do not specify their nature. For the towns a large solid black circle is used, and for 'other places' an open circle. That is in general conformity with existing practice elsewhere. For sites of special character the circle is retained with specific modifications. Thus, hill-forts are shown by an open circle with three small huts on its circumference, and cashels (fortified monasteries) by an open circle with interior niches on the circumference. (By a curious slip Bamborough, a hill-fort *par excellence*, is shown by an open circle; perhaps because it appeared as such on the overlapping part of the South Sheet, where no special symbol for hill-forts was used).

The hut-symbol is used also for lake-dwellings and crannogs, by an ingenious addition of legs to the village symbol. In contrast with the South Sheet there are no single huts. For the few earth-houses that can be certainly assigned to the period, a new and not very satisfactory symbol like a conventional flash of lightning has been adopted. (Could not the hut-symbol somehow be modified to represent what was, after all, only a kind of underground hut?) Burial-grounds are indicated, quite appropriately, by the same device as that used for its trade-mark by the London Underground. For symbol-stones one of the commonest of the symbols themselves (an inverted crescent with v-shaped rod) is used. The few stones with animals and nothing else carved on them are suggested by a minute bull and an equally minute wolf.

Thus throughout there is a consistent system, and it is evident that the symbols have been designed in such a way as generally to suggest the objects they represent. We have advanced a long way from the days when archaeologists played noughts and crosses on half-sheets of notepaper. May we hope that, on the next edition of the O.S. map of Roman Britain, the symbol adopted for 'villas' will no longer be one that suggests a well-known brand of beer?

EARLY SCOTLAND

Of the printing we can only say that it is in keeping with the high standards set for over a century by the Ordnance Survey. It cannot have been easy to combine graceful colouring with legibility, particularly in the intricate mesh of land and sea on the west, and in the narrow defiles of the interior. As a whole, the map gives an admirable picture of North Britain and its adjacent islands—a narrow tongue of land projecting into the cold waters of the North Atlantic. The marking of the Orkney and Shetland Isles in their correct cartographical position (and not as an inset, for which we are duly grateful) involves the inclusion also of much blue sea ; but this is really an advantage, for not only does it make possible the showing of the remote St. Kilda and the even remoter island of North Rona (with its cashel), but it shows also the edge of the continental shelf, with its blue depths. It is a little unfortunate that the table of reference had to come here, but there is no other obvious place for it.

Finally, a word about the cover. It is a work of art which cannot be praised too highly. It succeeds in conveying exactly the right impression, being composed of elements of the contemporary art skilfully blended into a homogeneous design. The rather bibulous bearded warrior with his empty drinking horn is quite in keeping with the spirit of the times ; and if his pony appears to be climbing a mountain that may be because he came from the highlands—or it may be an optical illusion. The border, of vine-scrolls, Northumbrian and Pictish, is used also with good effect for the margin of the map and of the table of reference. Merely as a 'selling-point' the cover has considerable merit, for who will not be tempted by it to look inside ?

So far we have dealt rather fully with the map *as* a map, because this aspect, we think, deserves fuller appreciation than its predecessors have received. With regard to its content one could say much ; but the severest criticism, we think, would be that future editions will probably contain more sites. The compiler has erred if at all rather on the side of caution, and that is quite proper in a publication that is produced by a Government Department. There are so many places in Scotland that positively reek of antiquity, and that one feels convinced belong to the Dark Ages ; but a subjective conviction of age cannot take the place of historical evidence. Such places as Dull, Dunblane, Stirling, Kilcase (Ayr) were surely important before the Union of the Kingdoms ; yet, though mentioned in the Introduction, they do not appear on the map, nor, in the present rather backward state of research *ad rem* do we see how they could. Perhaps, since

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these were omitted, it was a little venturesome to include Banchory, though there is evidence of a kind for it. Some of the identifications, too, are a little shaky (*e.g.* Crog Reth, Tula Amain) though here again a case can be made out. Quite the most striking omission is that of Edinburgh. The evidence of the old Welsh poems seems conclusive; but it should be remembered that, at the time when the map was compiled, Professor Ifor Williams' great book had not yet appeared;* and there is a tantalizing uncertainty about the early forms.

Some interesting sites appear on the overlap. Since 1935 Camlann has advanced in status, it seems, from a bold hypothesis to an officially recognized fact; and Arderit has reverted, as it should, to the much better old form Armterid. The important battle of Daegsan Stane is definitely located at the Dawstone Burn; there will still remain however an element of uncertainty until early forms of the burn are discovered.

Quite a number of the identifications are new, in the sense that they have never before appeared on a map; and for many of them the excellent authority of Professor Watson is cited. Some of these are open to discussion (but what is not, in Dark Age Scotland?). Most, if not all, are justified here if the map is regarded, as it should be, as a graphic statement of orthodox opinion at the present date. For, in the present neglected state of early Scottish history, orthodox opinion resolves itself into little more than that of some half dozen scholars in Britain and fewer still on the Continent. Moreover the map is the first of its kind ever published, and a beginning had to be made, if only to stimulate criticism. On the whole we think that the selection of names and the identifications adopted are the best that could be expected in the circumstances. The majority are beyond cavil.

We have no room to discuss the distribution of symbol-stones at all adequately. Perhaps that is fortunate, for we should only be led, by way of the introduction, into a discussion of the Picts and their possible—or as some think impossible—association with these stones. Let it suffice to call attention to the concentration of the symbol-stones in the valleys of the Spey and of the Don and its tributaries (a fact concealed by previous small scale sketch-maps) with a thinning out north and south; and to evidence of progressive southerly drift shown by the diagrams on pages 32-4. This is in complete agreement with the known historical shift of 'capitals' from near Inverness to the neighbourhood of Perth (Scone and Forteviot) and eventually of course

* See review in this number, pp. 25 ff.—EDITOR.

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to Stirling and Edinburgh. In passing we would utter a plea for a study of these symbol-stones in the light of modern knowledge ; and to do so is in no way to disparage Romilly Allen's magnificent corpus. They are the finest surviving relics of the only truly Scottish art, and they deserve more, both in study and conservation, than they have received.

Finally, this is the only adequate historical map of Northern Ireland that has yet appeared. It is perhaps in keeping with tradition that it should appear as part of a map of Scotland and be published in England. We are not competent to criticize it, but we hope that in spite of these inevitable handicaps, it will not be overlooked by the lively schools of archaeologists in Ireland. For it uses archaeological as well as historical evidence. And this suggests one last criticism, not so much of the map itself as of the background of research that it represents. There must be several hundred sites at least that were inhabited during the Dark Ages, which are visible and in evidence today but have not been excavated and consequently have had to be omitted. What of Birrens (Carruthers) in Dumfriesshire for instance, suggested by the writer in the Royal Commission's Report as the headquarters of Riderch Hael and a most likely earthwork for such ? (see Introduction pp. 6, 7). Excavation is badly needed here and elsewhere. But there must be besides many sites that have already been excavated (especially crannogs) and yielded Dark Age objects, that, for lack of archaeological knowledge, cannot yet safely be identified as such. Given a few securely dated types (of implements and pottery) these sites would come into their own, and be available for the next edition, with no more than a little study in museums and records. Such work is well suited for a thesis or prize essay.

In conclusion we can only hope that the present edition will be rapidly exhausted ; and even if the next few editions are necessarily little more than reprints, in the course of time some approach to completeness will surely be achieved. This depends, not upon the Ordnance Survey but upon the activities of Scottish scholars and archaeologists.

Iron Age Camps in northwestern France and southwestern Britain¹

by R. E. M. WHEELER

IN recent years considerable attention has been devoted to the problems of the Early Iron Age in the British Isles; and, amongst these problems, that of the relationship between the insular and the continental cultures of the period has not become simpler or clearer as the British evidence has accumulated. How far, and in what manner, were the various Iron Age cultures of Britain derived from the continent? How far, and under what conditions, were they due to local initiative in Britain itself? Until questions such as these can be answered approximately, it will remain impossible alike to estimate the real achievement of the later prehistoric civilization of the island and to visualize the full significance of the adjacent civilization of northwestern Europe. The problem is not an easy one. The agricultural and therefore local basis of most of the Iron Age economy of Britain encouraged the strong local differentiation of cultural forms, and this local individuality was enhanced by the fashion in which the major tracts of open and habitable chalk or greensand tended, in ancient times, to be isolated by expanses of dense and often impassable forest. And, similarly, an intrusive element from overseas might easily take root in a particular area of southern or eastern Britain without directly affecting other areas within a relatively short map-distance. Indeed,

¹ This interim report is substantially the English version of the French summary which will appear in the *Revue archéologique*, and was written therefore from the point of view of the French reader. The complete report will be published by the Society of Antiquaries of London, which was responsible for the initiation of the work. The expedition was rendered possible by a generous Leverhulme Research Grant supplemented by the University of London and the Society of Antiquaries, and consisted of about 60 members of British and other universities. Miss K. M. Richardson was a co-director of it, and the basis of the work was a survey of French museums carried out in 1935 by Miss Leslie Scott, who also supervised the excavation of Kercaradec in 1938. The actual excavation of the other two sites was controlled largely by Mr W. Wedlake, and the photographer was Mr M. B. Cookson.

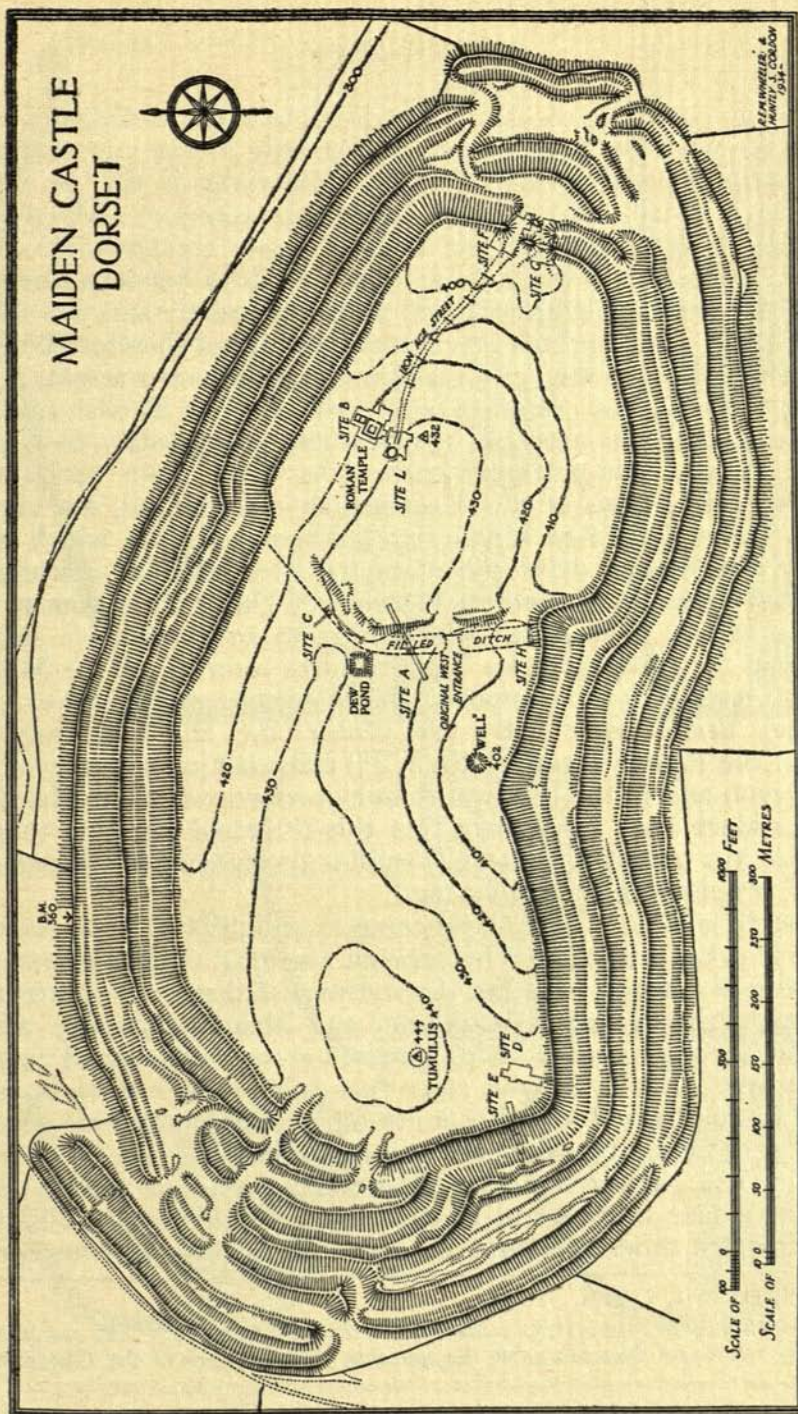


FIG. 1
By permission of the Society of Antiquaries of London

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scarcely a year passes by without the addition to our knowledge of some new local British Iron Age culture bearing only partial and uncertain affinities with those previously known. Especially is this so where pottery—as is usually the case—is the main criterion. Metalwork, representing both a superior and a more easily transportable craft, might be expected to provide more extensive links between one area and another; but friable ornamented metalwork rarely survives in the acid soils of the chalk and the greensand, and cannot therefore be used by the archaeologist to the degree which its importance warrants.

In 1931 Mr C. F. C. Hawkes (as is well known to English readers) introduced a welcome measure of order into this medley of British Iron Age cultures by grouping them into three main categories: A, B and C.² The basis of this classification was cultural, not chronological. Under Iron Age A was grouped those cultures which were essentially a provincial development of the later Hallstatt cultures of northeastern France and western Germany. Under Iron Age B was grouped a variety of cultures which were to an appreciable degree derived from the continental La Tène cultures centring on the Marne. Iron Age C comprises the composite Belgic culture which first entered southeastern Britain early in the first century B.C. and ultimately (by the time of the Roman invasion of A.D. 43) extended its influence in the west to Devon and in the north to the neighbourhood of the Humber. Experience since 1931 has shown that this threefold division, though inapplicable in a minority of cases, is valid and convenient in a majority of cases if properly and carefully used.

Of the three categories the most complex and difficult is the second. When Mr Hawkes propounded his scheme, the most notable element in the Iron Age B category was the decorative and distinctive pottery of the Somerset lake-villages, Glastonbury and Meare—a pottery which Déchelette long ago compared with a limited group of decorated pottery from Brittany.³ But it is now clear that a large variety of ceramic groups in Britain have an equal claim to be included under this general heading, *i.e.* as falling between the ultimate Hallstatt group (A) and the Belgic group (C). These B groups differ widely from one another both in their immediate origins and in their local developments, but they include, amongst other elements, all wares which bear any vestige of

² ANTIQUITY, 1931, v, 60 ff.

³ *Revue archéologique*, 1901, II, 51; *Manuel d'archéologie*, II, 1467. The comparison is only partly valid, and does not solve the question of the origins of the Glastonbury ware.

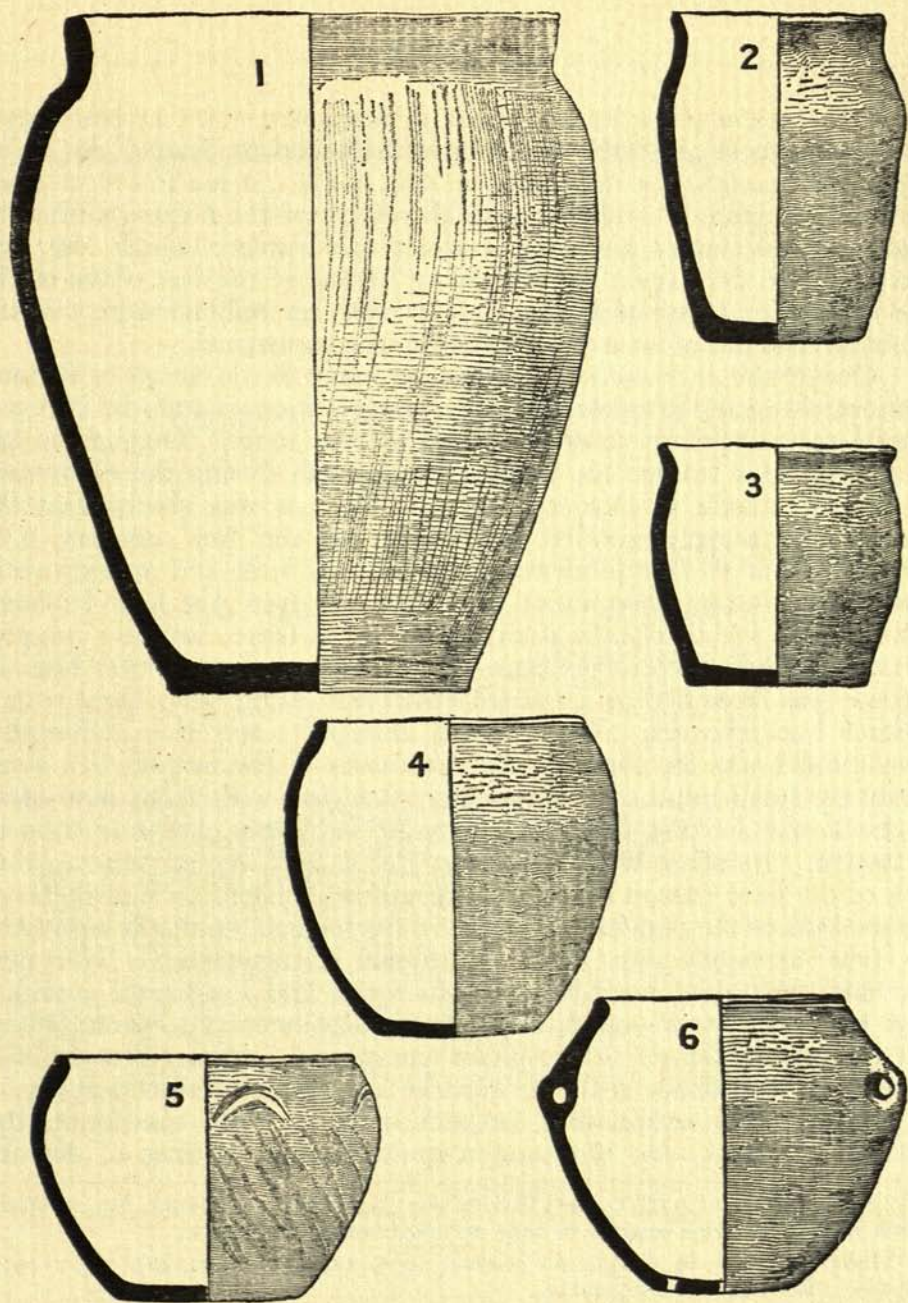


FIG. 2. POTTERY FROM MAIDEN CASTLE, DORSET (½)
 1-3, Iron Age A pottery, 3rd—2nd centuries B.C. ; 4-6, Iron Age B pottery, 1st centuries B.C.—A.D.
By permission of the Society of Antiquaries of London

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La Tène ornament—a feature never, it seems, found on an Iron Age A pot—and almost all wares which have the rolled or beaded rim of La Tène metalwork and pottery (FIG. 2, nos. 4–6).⁴ It has in recent years become increasingly evident that with certain of the B groups must be associated distinctive methods of attack and defence which have left their mark widely upon the camps or *oppida* of the last phase of the pre-Roman Iron Age of Britain. It is with this military aspect of the problem that the present report is primarily concerned.

One of the characteristic features of many of the camps of Britain, particularly in the southwest, is the multiplication of lines of defence, extending in depth to 50 or 100 yards or even more. During the last four years, this feature has been studied afresh during the excavation of Maiden Castle in Dorset (FIG. 1).⁵ Here it was shown that the outer lines were a sudden innovation of the first century B.C., and that prior to this innovation the hillfort, built and subsequently extended by a large population possessing an Iron Age A or Hallstatt culture, had included only a single line of defence, save for supplementary screens at the entrances. The addition of two outer lines of defence was identified as a sudden structural change associated with a definite but relatively slow cultural change;⁶ and this differential development was ascribed by the excavators to the arrival of a comparatively small number of newcomers bringing with them new ideas of attack and defence, but not accompanied by any extensive train of craftsmen. In other words, Maiden Castle and, by inference, other sites of the kind passed at this time under a new and foreign dynasty, but the bulk of the population remained unchanged, gradually assimilating new elements into their traditional craftsmanship. The new elements—including notably bead-rim bowls and the use of counter-sunk handles (FIG. 2)—gradually became dominant and characteristic, and I have christened the resultant composite culture 'Wessex hillfort B'⁷ in accordance with the general terminology outlined above.

Now it is an axiom that methods of defence are conditioned by methods of attack, and the sudden multiplication of lines of defence

⁴ The beaded rim is of course a feature also of Hallstatt metalwork, but its transference to pottery seems scarcely to have occurred before La Tène I.

⁵ Interim reports in *Antiquaries Journal*, 1935, xv, 265; 1936, xvi, 265; 1937, xvii, 261; final report in preparation.

⁶ *Antiquaries Journal*, 1937, xvii, 275.

⁷ The term 'Wessex' is here used as equivalent to Dorset, southern Somerset, eastern Devon, western Hampshire as far as the Avon, and southwestern Wiltshire.

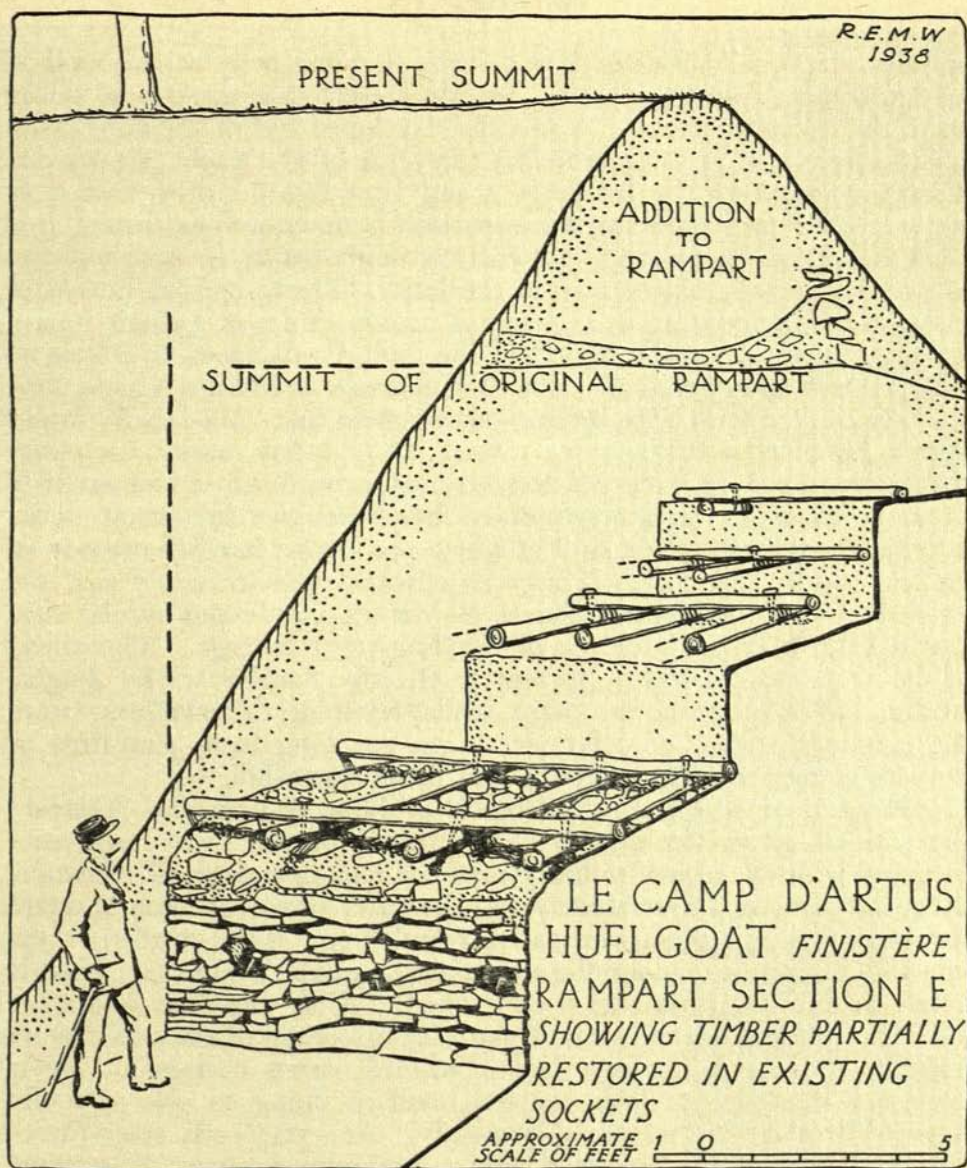


FIG. 3 (see p. 66)

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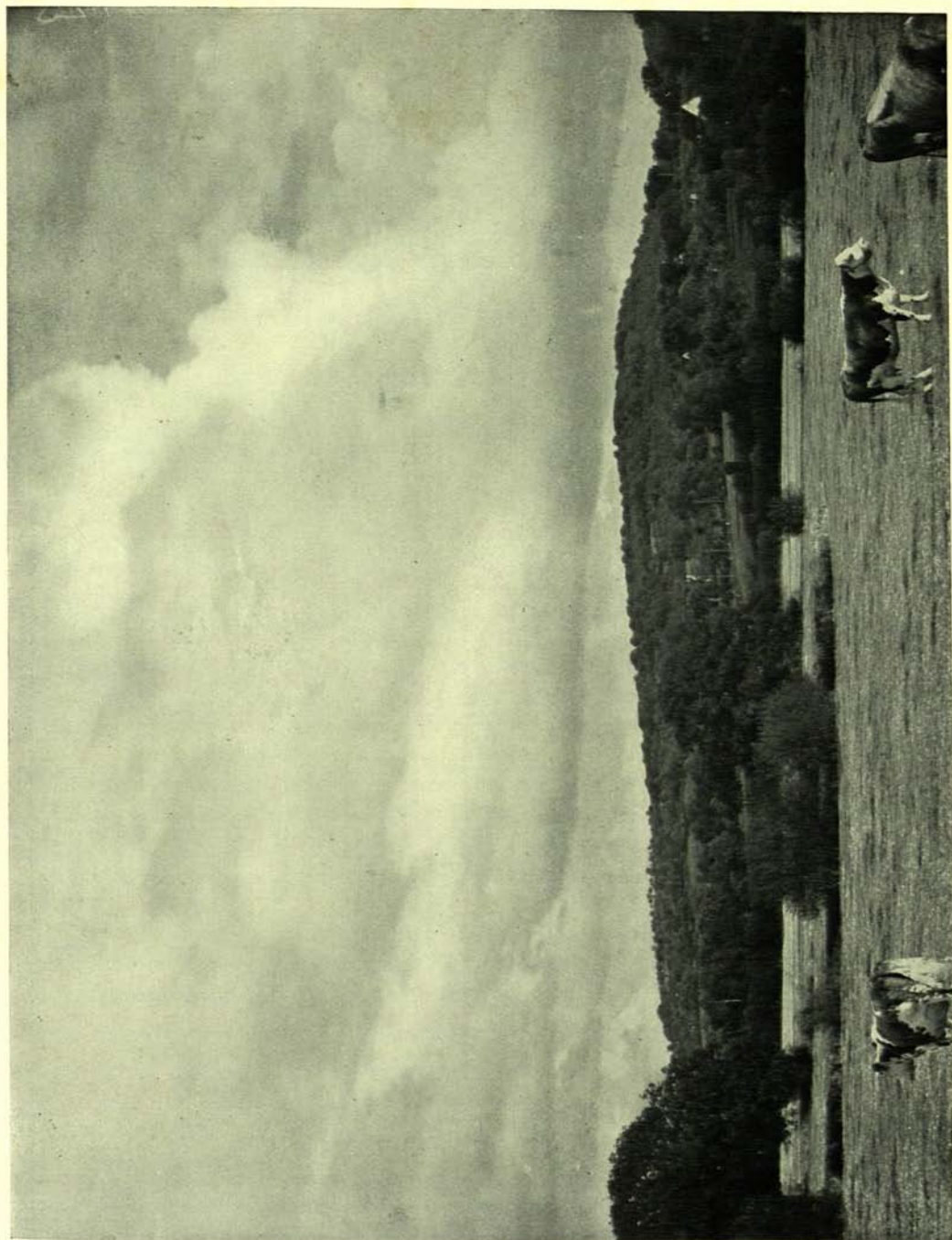
implies the equally sudden introduction of some new tactical method or some new weapon (or both). In the present case there is no doubt as to the causative factor: it was the developed use of the sling, with an effective range of something like 100 yards on the level. At Maiden Castle, where both the Iron Age A and Iron Age B cultures are very abundantly represented and a comparison is therefore well based, it is seen that the sling, although not entirely unknown in the earlier phase, is overwhelmingly represented in the later. The numerous hoards of sling-stones, amounting to as many as 22,000 stones in a single hoard, belong exclusively to the period of the multiple defences, and it is no exaggeration to say that the Iron Age B levels at Maiden Castle were literally riddled with sling-stones—so much so that 'hill-fort B' might almost be called a 'sling-stone culture'. In a few cases these sling-stones are of clay of the usual pointed-oval form, but in a vast majority of cases they are naturally-rounded beach-pebbles somewhat under 2 oz. in weight and of the kind of which the Dorset beaches provide an endless supply. In brief, it may be affirmed with certainty that the extension in depth of the defences of Maiden Castle and similar sites was the direct reply to the use of the sling-stone barrage. The object of the new defences was to prevent an effective mass-attack by slingers working up-hill towards the camp, whilst retaining the attackers within the maximum range of defenders using the sling from platforms or terraces along the main ramparts and firing down-hill.⁸

Here, then, was an archaeological problem of first-class interest; the sudden arrival in southwestern Britain of dominant minorities bringing with them new military methods and new cultural elements, the former imposed immediately and the latter slowly diffused through the numerically preponderant native population. Between them, these innovations were to colour the whole background of the early historic period within the wide region concerned. What was their origin?

The suddenness and instant maturity of certain of the new factors make it impossible to postulate a wholly native derivation. It is necessary therefore to look overseas, and in doing so one naturally turns to Brittany and western Normandy. At a very early stage therefore in the work at Maiden Castle a preliminary search was made

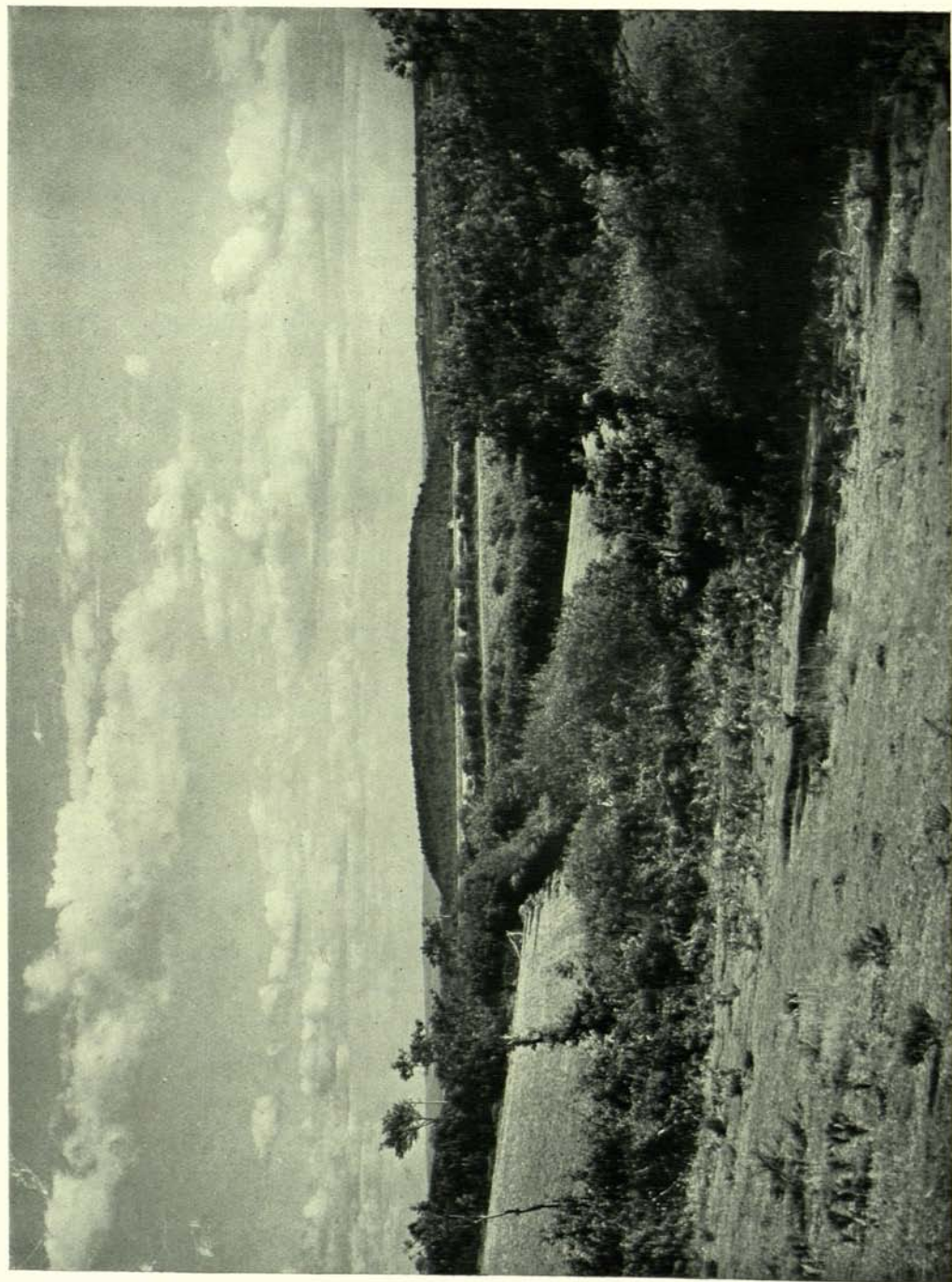
⁸ It is not here maintained that slingstones are everywhere associated exclusively with multiple defences. The general equation is claimed only for northern France and Britain; elsewhere, single lines of ramparts are sometimes associated with slingers, and it is certain that even in the regions now in question some single-ramparted camps were still used in the 'slingstone period'.

PLATE I



LE CHÂTELLIER, LE PETIT CELLAND: DISTANT VIEW FROM THE NORTHWEST (see p. 67)

PLATE II



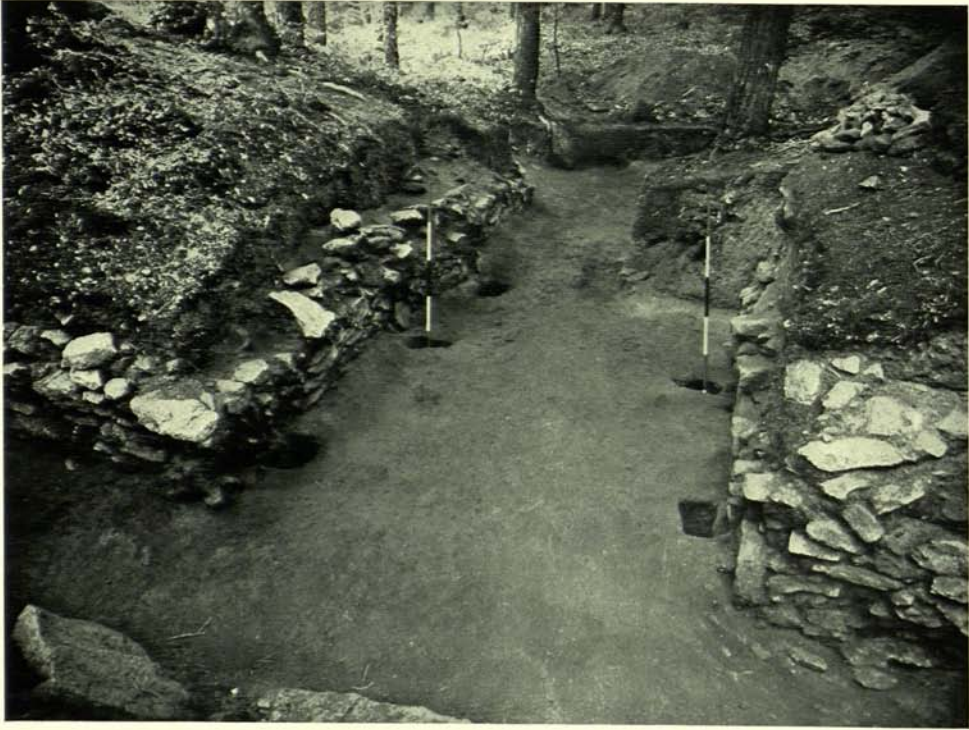
LE CAMP D'ARTUS (CENTRAL BACKGROUND) FROM THE NORTH (see p. 65)

PLATE III



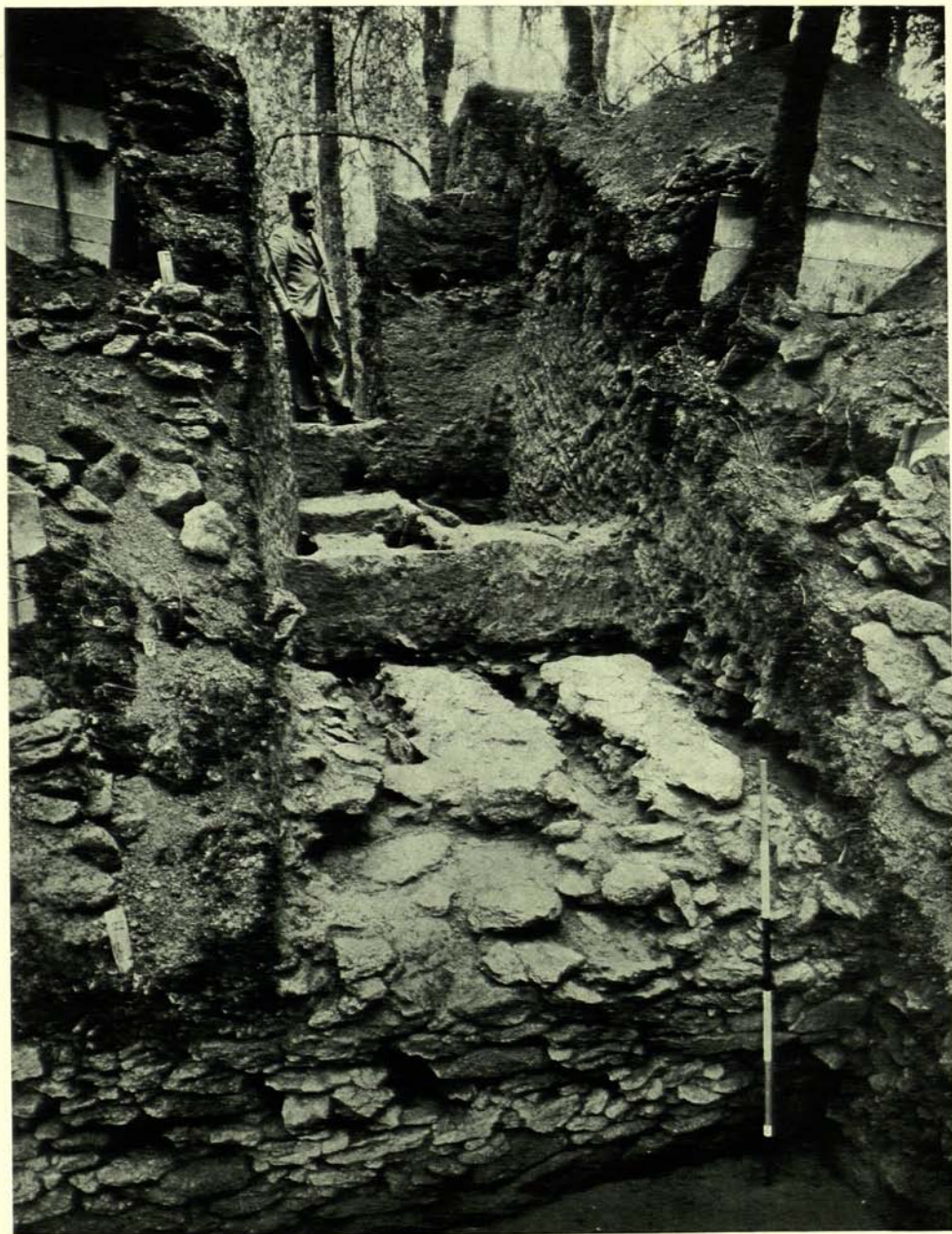
LE CAMP D'ARTUS: MAIN (NORTHEAST) ENTRANCE, AS EXCAVATED (see p. 66)

PLATE IV



LE CAMP D'ARTUS: SOUTHEAST ENTRANCE, AS EXCAVATED (see p. 66)

PLATE V



LE CAMP D'ARTUS: SITE E, SECTION THROUGH *MURUS GALLICUS* (see p. 66)

The line of the original summit can be seen opposite the middle of the figure

PLATE VI

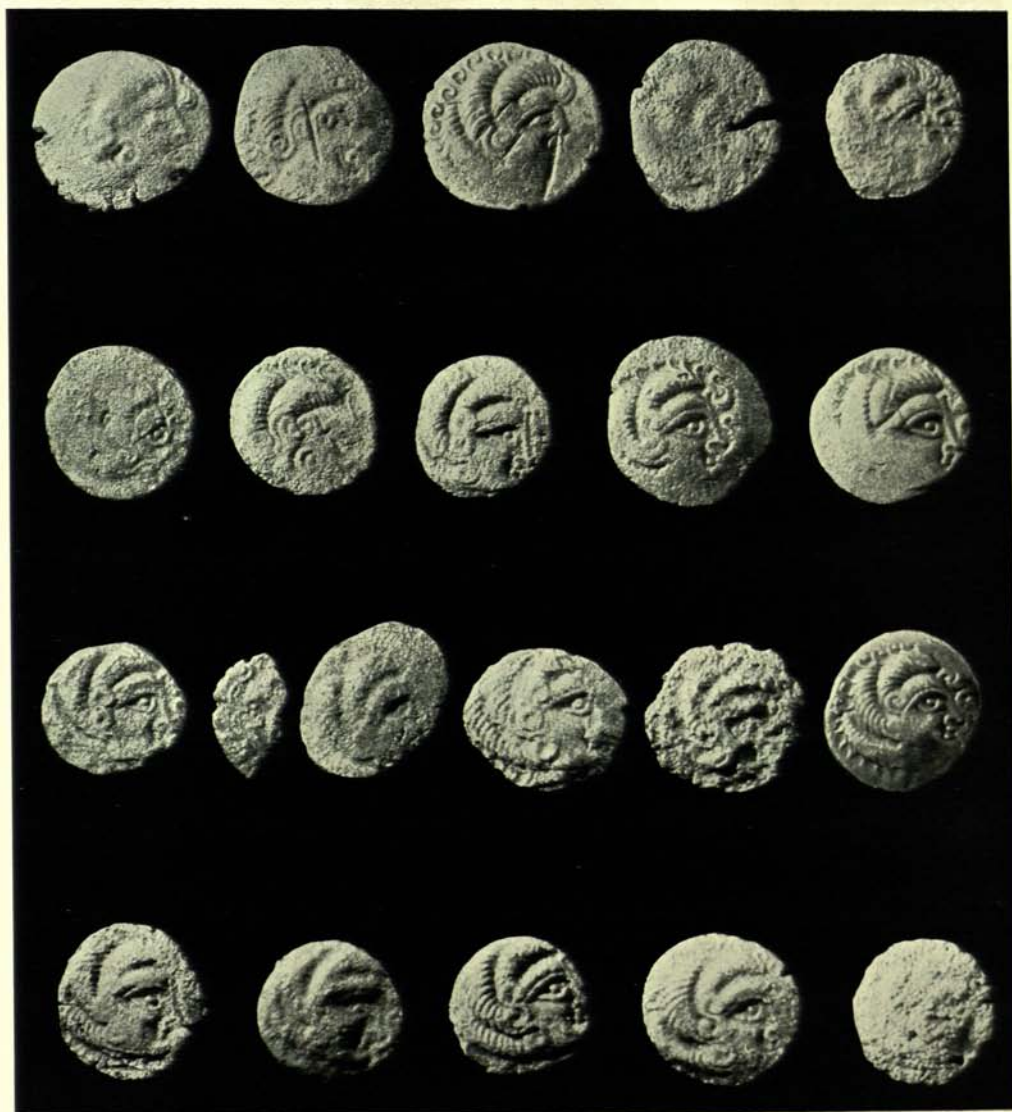


LE CAMP D'ARTUS : *MURUS GALLICUS*, SITE E (see p. 66)



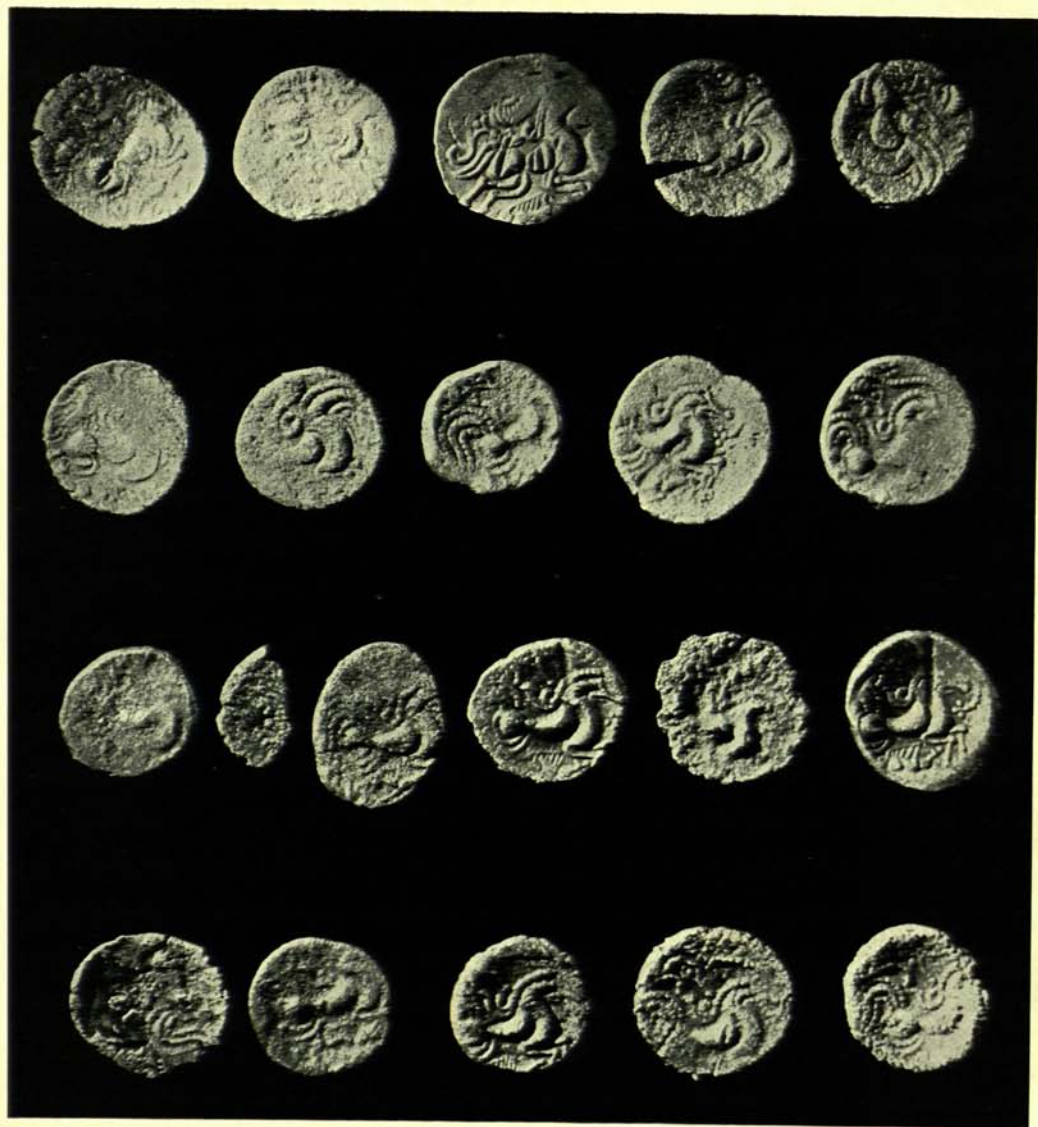
A BRETON CLIFF-CASTLE : CASTEL MEUR, KERNOT, FINISTÈRE (see p. 70)
The three figures are standing on the three ramparts

PLATE VII



LE CHÂTELLIER, LE PETIT CELLAND : GAULISH COINS, OBVERSES (1) (see p. 67)

PLATE VIII



LE CHÂTELLIER, LE PETIT CELLAND: GAULISH COINS, REVERSES ($\frac{1}{2}$) (see p. 67)

IRON AGE CAMPS IN FRANCE AND BRITAIN

throughout the museums of northwestern France for cultural elements analogous to our Wessex hill-fort B and the adjacent contemporary cultures. The preliminary survey made it clear that in the available museum-material from northern France only rare and partial analogies to the British material were forthcoming. Moreover, in spite of useful local surveys,⁹ insufficient evidence had been collected for the preparation of a distribution-map of the various types of camp in the region, and scarcely a single example had been excavated by modern methods. Accordingly it was resolved to approach our French colleagues for permission to carry out a programme of ground-survey and trial-excavation, with our special problems in mind.

As the result of a preliminary survey by Mr C. A. Raleigh Radford (Director of the British School at Rome) and myself, two sites were selected for trial. Both sites were distinguished from most others in the region by the possession, in part, of more than one line of defensive ditch. In both cases the outer lines were of feeble dimensions compared with those of many of the camps of southwestern Britain; nevertheless, in the northern part of the region in question they were the only sites of appreciable size which could be brought into any sort of structural relationship with them. The two sites were the Camp d'Artus at Huelgoat in the midst of Finistère, and the Châtellier, close to Le Petit Celland, 10 kms. east of Avranches, Manche. Their exploration was rendered possible by the ready good-will of M. Raymond Lantier, the Ministère des Beaux-Arts and the Département des Eaux et Forêts, Madame la Comtesse de Belloy de Bouéxic, M. J. Seguin of Avranches and, not least, by Dr Claude Schaeffer who visited the work in progress.

The Camp d'Artus at Huelgoat (FIGS. 3, 4; PLATES II-VI) is 75 acres in extent within the defences and, with one possible exception, is by far the largest camp in the three northwestern departments of France. It lies in the midst of the former territory of the Osismii, who occupied roughly central and northern Finistère and the western part of the Côtes-du-Nord. In spite of the unimposing character of its outworks, it is the only camp in that region which may

⁹ Notably P. du Chatellier, *Les époques préhistoriques et gauloises dans le Finistère* (Rennes, 1907); C. Délandre, *Le Morbihan, son histoire et ses monuments* (Vannes, 1847); F. de la Messelière, 'De l'âge probable des châteaux de terre des Côtes-du-Nord', *Société d'émulation des Côtes-du-Nord, Bulletins et mémoires*, LXV, 51 ff. (S. Briec, 1934); P. Banéat, *Le Département d'Ille-et-Vilaine*, I-IV (Rennes, 1927, etc.); and the reports of the Commission d'étude des enceintes préhistoriques de la Société Préhistorique Française.

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be said to have more than a single line of defence. Excluding the medieval motte which has been inserted into its northern end, it falls structurally into two parts, of which the northern and smaller represents a curtailment and partial reinforcement. The original rampart is found by excavation to have been of *muris Gallicus* construction, the earthen bank being laced by nailed timbers and faced externally by a stone wall which incorporated the projecting ends of some of them. Where, in the northern part of the camp, the rampart was slightly reinforced at the period of curtailment the original height of the *muris Gallicus* rampart is preserved beneath the additional material and can be seen to have been 12 ft.—probably the only known instance where the original height of a rampart of this kind can be ascertained with certainty (see FIG. 3). The new cross-rampart inserted in the process of curtailment is more roughly built and has a stone external face without timber lacing.

Two entrances were completely excavated—the main northeastern gate and a smaller gate at the southern end. Both entrances were in-turned, and the stone revetments of their flanks had been reinforced by vertical posts, partly to carry the single-span gates and partly perhaps to carry a bridge or tower over the passage. At the southern entrance there was evidence of violent destruction; a charred gate-timber lay on the original surface, and with and over it the walls had been thrown down upon that surface without intermediate accumulation.

Within the large area of the camp many small trial-sites were opened up amongst the close-set trees of the pine forest which now covers the whole hill. A number of these sites produced pottery, in one case associated with a Gaulish coin of a type already well-known from the area of the Osismii. In most cases, the occupation-debris was confined to a single layer, and throughout the camp the pottery forms a uniform complex. It includes, among coarser wares and fragments of Roman amphora, a certain number of what may be called Ultimate Marnian types; and the whole complex may be regarded as a western counterpart of the groups which further east would be associated with the Belgae.

The restricted and uniform occupation induces the inference that the site, in spite of its two structural phases, was not long inhabited. A single generation would, at a rough estimate, amply produce the evidence recovered and the period may well have been less. Before considering the historical context further, something must be said of the second site, Le Châtellier, near Avranches.

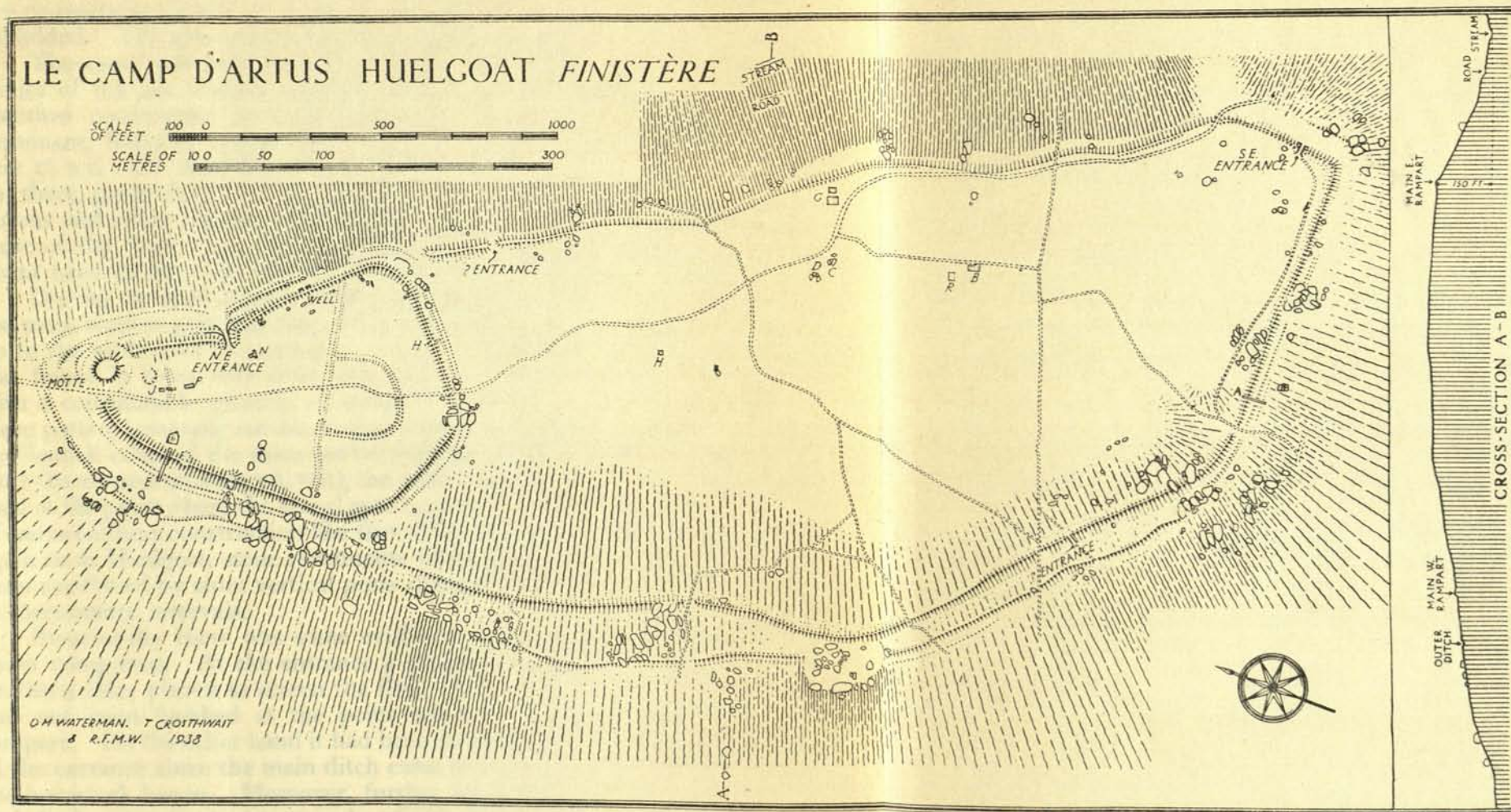


FIG. 4 (see p. 65)

IRON AGE CAMPS IN FRANCE AND BRITAIN

Le Châtellier (FIG. 5 ; PLATE I) occupies the rounded and steep-sided end of a promontory projecting westwards of Le Petit Celland. For the most part the defences consist of a single rampart and ditch with low counterscarp bank, but towards the south an outer bank and ditch are added. The area enclosed by the defences is 48 acres.

The camp has been known to local antiquaries at least since the forties of the last century and has been the centre of prolonged and pointless controversy in connexion with the campaign of Caesar's lieutenant, Sabinus, against the Unelli and neighbouring tribes in the year 56 B.C. On more than one occasion excavations have been carried out there, particularly by E. le Héricher in 1862. Gaulish coins, rotary querns and other objects were found during this and other work, but most of the finds were destroyed when the Avranches Museum was burnt towards the close of the nineteenth century.

During the past summer, sections were cut through the defences, the main eastern gate was examined, and small trial-areas were opened up in the interior of the enclosure. Very little evidence of occupation was found in these trial-areas, but such as it was it equated exactly with a considerable quantity of material recovered from the entrance. Here pottery generally similar to that found at Huelgoat, together with 19 Gaulish coins of the same period (middle of the first century B.C.), were discovered (PLATES VII, VIII), the coins forming a cluster across the road at the point where the actual gate had stood. The main rampart was of *muris Gallicus* construction like that of Huelgoat, and in the entrance, again as at Huelgoat, stone revetments which flanked the roadway had been reinforced by stout vertical posts clamped and nailed into position at three-metre intervals.

Structurally there was some evidence that the fortifications were never completed. At the entrance a supplementary hornwork on the southern side, placed to screen the flank of the incurved passage-way, had not been finished at the point where it approached the main rampart. On the other hand it had been included in the original design of the entrance since the main ditch came to an end at the point where the hornwork began. Moreover, further south the outer line of bank and ditch came to an abrupt and unfinished end as it approached the level summit of the ridge, where reinforcement would be most necessary.

This incompleteness, combined with the uniformity and extremely restricted character of the occupation of the site, indicates that it was held for a very short space of time, and the general evidence of the coins and pottery is that that time cannot have been very far removed from the

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middle of the first century B.C. Moreover, the end of the *oppidum* was associated with violent destruction; charred timbering was found on the site of the gate, and the debris of the walls and ramparts lay on the road-surface. In other words, the evidence from Le Petit Celland tallies generally with that from Huelgoat, save that Huelgoat with its two structural phases hints at a slightly more prolonged occupation.

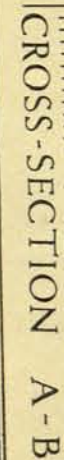
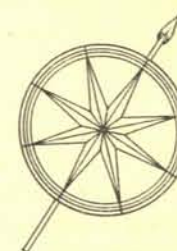
Before considering the two *oppida* in a wider context, a further structural feature common to them both demands comment. Reference has been made to an outer line or lines of defence as a controlling factor in our choice of these earthworks for excavation. It should again be emphasized, however, that these outworks are of restricted extent and size and are not closely comparable to the strong multiple works of many of our British hill-forts. It is clear that the builders of the camps at Huelgoat and Le Petit Celland regarded the main rampart as the essence of the plan, adding outworks spasmodically and unconvincingly. And incidentally, in neither camp was a single sling-stone found. In other words, we are here confronted with something essentially different from the 'sling-stone culture' of southwestern Britain and (as we shall see below) of southern Brittany.

Now for the wider context. First, the Huelgoat *oppidum* stands out by reason of its size and elaborateness in a region which otherwise contains few and small hill-forts. Finistère is naturally an unfertile region of rock and scrub and was not developed agriculturally until the nineteenth century; whilst today farmers complain that it is 'mauvais pour l'agriculture'. In such circumstances, so large an aggregation as is suggested by the Camp d'Artus is economically entirely out of place, even if all due allowance be made for the propinquity of silver-lead mines; nor can it be said that the size of the *oppidum* is determined by the nature of its site, for the western defences are carried far down the hillside, clearly with a view to extending the area enclosed to the uttermost limit. In short, we are confronted with a monstrosity which can only be the product of some exceptional political or military crisis, when it became suddenly necessary for the tribal units to coalesce at a focal point for self-preservation.

At Le Petit Celland the picture is essentially similar. Here the countryside is somewhat more fertile than that of Finistère, and certain of the *oppida* of the Manche are rather larger than those of the west, but here also the large and unfinished site is outstanding amongst the hill-forts of the whole region and suggests a sudden stress similar to that which we have recognized at Huelgoat.

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FIG. 5 (see p. 67)



IRON AGE CAMPS IN FRANCE AND BRITAIN

Furthermore, twenty miles south of Le Petit Celland, in the western fringe of the Forêt de Fougères, can be seen the remains of another large *oppidum*,¹⁰ again some 50 acres in extent, which far exceeds in size any other camp of the region and is patently unfinished. The structure of its rampart consists of an earthen bank with stone facing, generally similar to that at Le Petit Celland and Huelgoat, though whether the structure includes timber-lacing can only be ascertained by excavation. Inspection of the visible remains, however, leaves little doubt that here is another of these large refuges constructed by an aggregation of tribesmen under stress and never completed.

Now, historically, two episodes between the end of the second century B.C. and the middle of the first might account for this crystallization of the tribal units of northwestern France. In the last years of the second century B.C. France was ravaged by the Cimbri and their allies and only the Belgic tribes east of the Seine were able to withstand them. Secondly, in and about 56 B.C. Julius Caesar was beating down resistance in northwestern Gaul and he makes it clear that this resistance was both sturdy and organized.

Of these two alternative possibilities, the former may be ruled out definitely at Le Petit Celland, where the abundant coins associated with the pottery in the only layer of occupation cannot be ascribed to as early a date as the Cimbric invasion. On the other hand, they are entirely in place at the period of the Caesarian conquest. At Huelgoat, too, the uniformity of the material and its association with a similar coin is again in favour of the later alternative, although the occupation was somewhat more prolonged. It might be suggested that the site was first fortified under stress from the Cimbric incursions and that the curtailment and refortification should be ascribed to the Caesarian period; but it would be difficult, indeed impossible, to spread the single uniform layer of occupation over the half-century which this chronology would imply, and, if a historical context be sought for the reduced enclosure, it could far more easily be found in the momentary recrudescence of activity amongst the Armorican tribes in 51 B.C.

This much, however, is clear; both Huelgoat and Le Petit Celland—to say nothing of the unexcavated site in the Forêt de Fougères—were occupied in the period of the Caesarian invasion, and the construction of Le Petit Celland at least, from its manifest incompleteness, may be regarded with reasonable certainty as a

¹⁰ P. Banéat, *Le Département d'Ille-et-Vilaine*, 1928, II, 250.

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reaction to that invasion. It may be recalled that both at Le Petit Celland and at Huelgoat there is evidence of a violent destruction and burning which is again consistent with the events of 56 B.C.

So much in summary for the two main centres of excavation. Concurrently with this work the identification of hill-forts in the five northwestern departments of France (Finistère, Côtes-du-Nord, Morbihan, Ille-et-Vilaine and Manche) proceeded systematically. The scanty information available from archaeological sources was supplemented by local inquiry, and, whilst there must certainly be omissions in the schedule¹¹ resulting from this process of investigation, it may be affirmed that no site of importance is likely to have been overlooked.

The first result of this process of ground-survey is striking. With the partial exception of the two somewhat anomalous sites already described, multiple earthworks of the western British type are almost entirely absent from these five departments *save in one region*. That exceptional region is the coast-line and its immediate hinterland in southwestern Finistère and Morbihan (FIG. 6). Here also major multiple earthworks of the Wessex type are absent; but a series of coastal promontories and other hill-top sites in this region are protected by multiple systems of bank and ditch which bring them into line at least with the minor examples of the British series. Cliff-castles such as Castel Coz¹² and Castel Meur (PLATE VI) on the Finistère coast between Douarnenez and Le Pointe du Raz are identical in character with cliff-castles of Cornwall, and comparable earthworks occur elsewhere further south, *e.g.* near Audierne, near Quimper (this a hill-top not a promontory site), near Quimperlé, at S. Avé near Vannes, on the Isle de Croix, on Belle Isle, and near Pouliguen in Loire Inférieure.

The two sites first named were roughly excavated many years ago. The pottery from Castel Coz is now in the Quimper Museum and falls into two main groups—a late Hallstatt series and a series which includes a hint of the bead-rim pottery of our Wessex hill-fort B. The pottery from Castel Meur is stored with the Du Chatellier Collection in the Museum of St. Germain-en-Laye; it is not extensive, but, with medieval sherds, includes evidence of two cultures which may probably

¹¹ This schedule will be included in the full report. That part of it which relates to multiple earthworks is here illustrated by the map, FIG. 6. It is important to remember that the map, in so far as the British distribution is concerned, must be supposed to include a multitude of derivative multiple earthworks, some of them long subsequent to the first introduction of the type by Venetic settlers.

¹² Plan in *Archaeological Journal*, 1872, XXIX, 314.

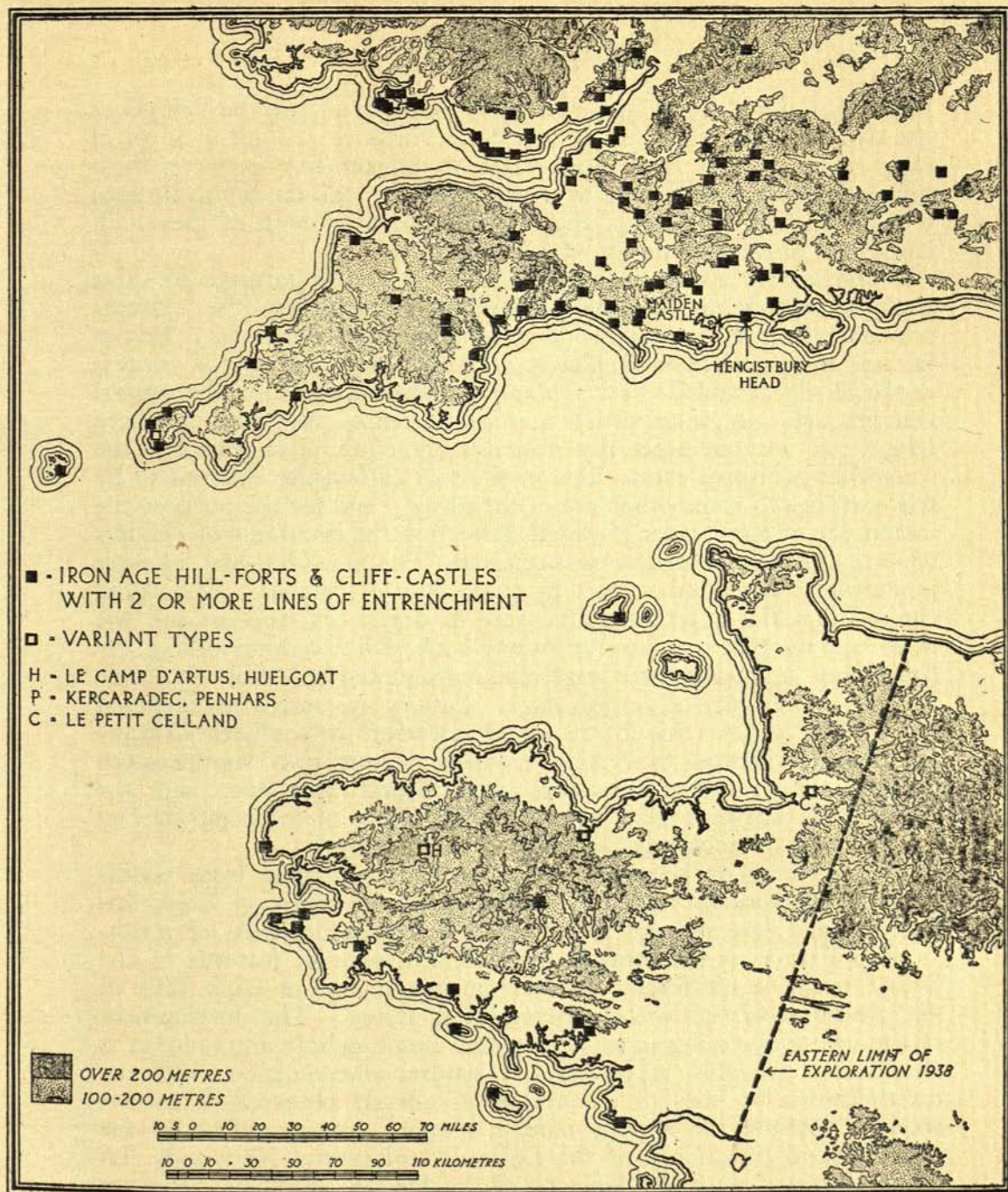


FIG. 6 (see p. 70)

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be correlated with those of Castel Coz. A more noteworthy fact about the two sites, however, is perhaps the occurrence in both of them of sling-stones, which is a further and significant link between these multiple defences and those of western Britain. So far as can be seen without further excavation, the triple ramparts of both of these cliff castles are of simple 'dump'-construction.

During our recent reconnaissance-work the importance of these multiple earthworks became increasingly apparent as the extreme scarcity of multiple earthworks elsewhere in northwestern France became more and more emphatic. The late date and special context of the Huelgoat and Le Petit Celland *oppida* removed from the general context the only noteworthy multiple defences near the northern fringe. It became especially desirable therefore to supplement the somewhat primitive excavations in the two cliff-castles referred to by trial-cuttings in some other site of the series; and for the purpose the inland site of Kercaradec (familiar name!) in the commune of Penhars close to Quimper (Finistère) was selected. This small hill-fort, $5\frac{1}{2}$ acres in extent, is defended in part by three lines of defence. Excavation showed that the outer ramparts were of dump-construction and the innermost rampart was faced front and back with dry-stone walling, the back being stepped in three stages and surmounted by a timber palisade along the inner edge of the top stage. Pottery was scarce but included one or two features which are found elsewhere in southern Brittany and can be related to certain features of our own southwestern ceramic. Numerous sling-stones were found here too, and the farmer said that in a small quarry in the centre of the camp he had found 'several thousands' of them. (PLAN, FIG. 7).

Here, then, on coastal or estuarine sites extending from southwestern Finistère through the Morbihan to the fringe of Loire Inférieure, and *here only* in and about the five departments of northwestern France, occur some at least of the essential features of the British multiple hill-forts; notably the use of dual or triple lines of defence in association with numerous sling-stones. The development of the use of the sling in an area where beach-pebble ammunition is abundantly available is in itself a natural phenomenon, and the multiplication of lines of defence is, as already observed, a natural sequel. Outside the regions named, a more cursory search of the region round the mouth of the Loire and of central Normandy has failed to reveal multiple defences of the kind in question. Further east, in the Seine valley, a series of large *oppida* sometimes exhibit the

KERCARADEC, PENHARS *NEAR* QUIMPER *FINISTÈRE*

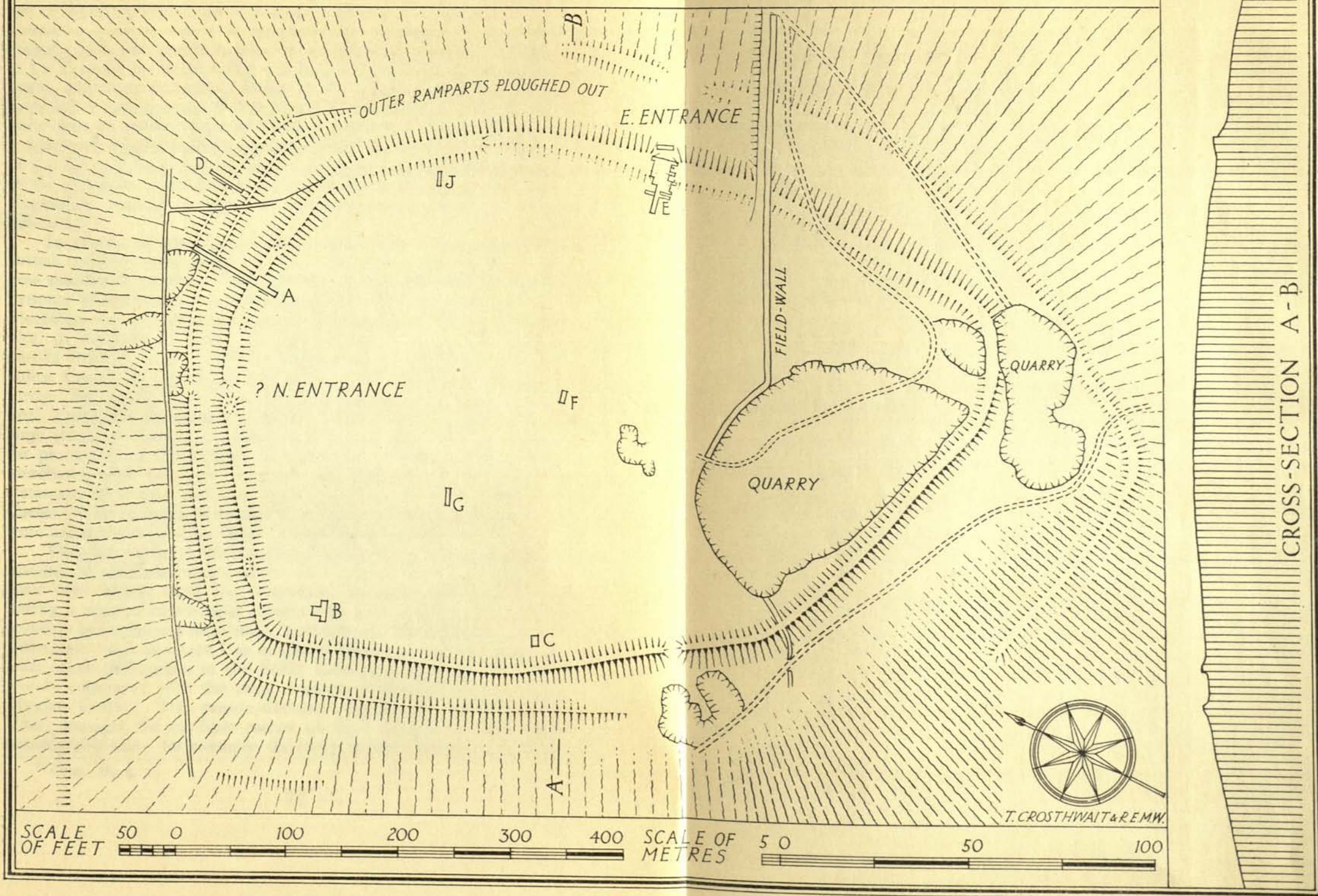
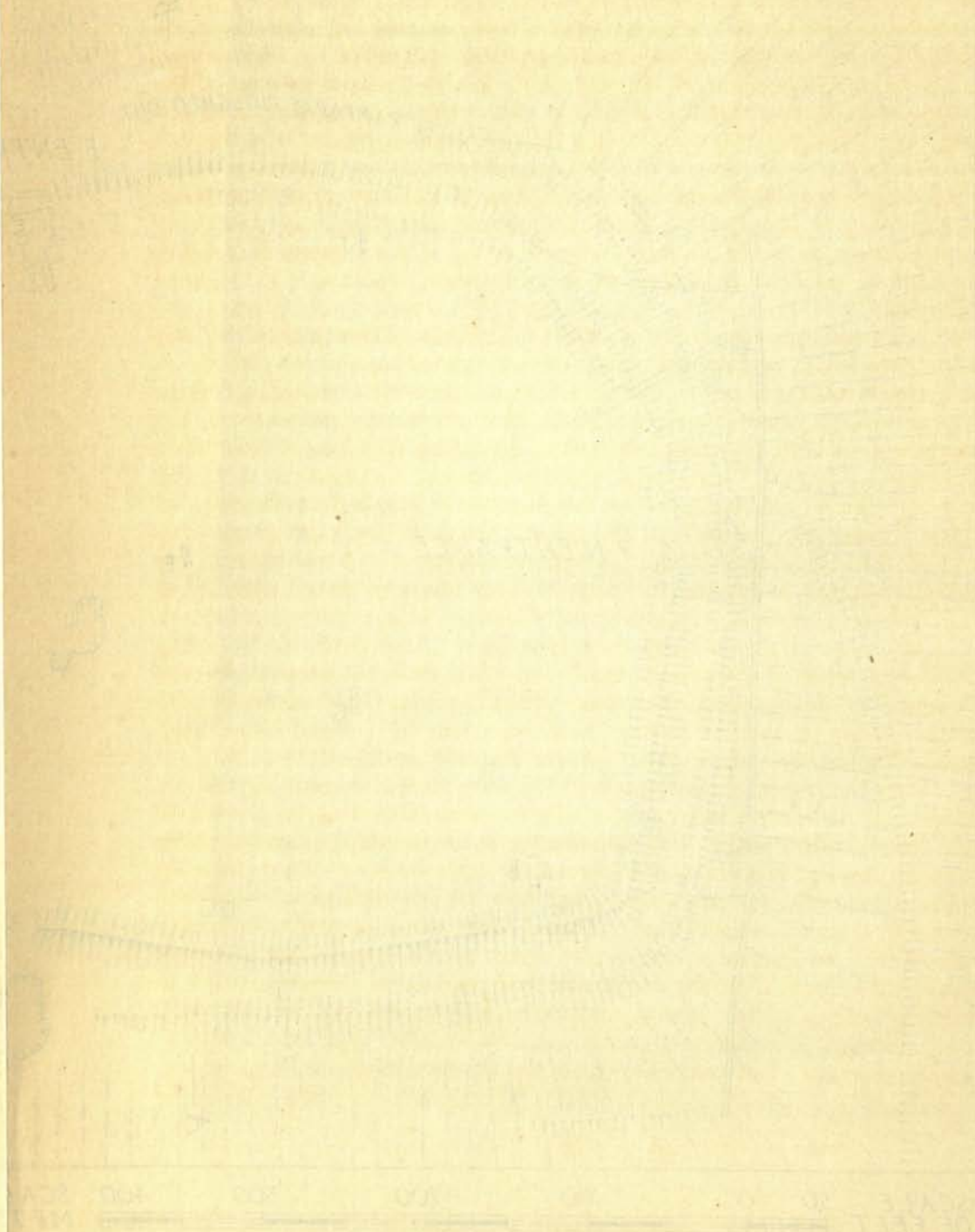
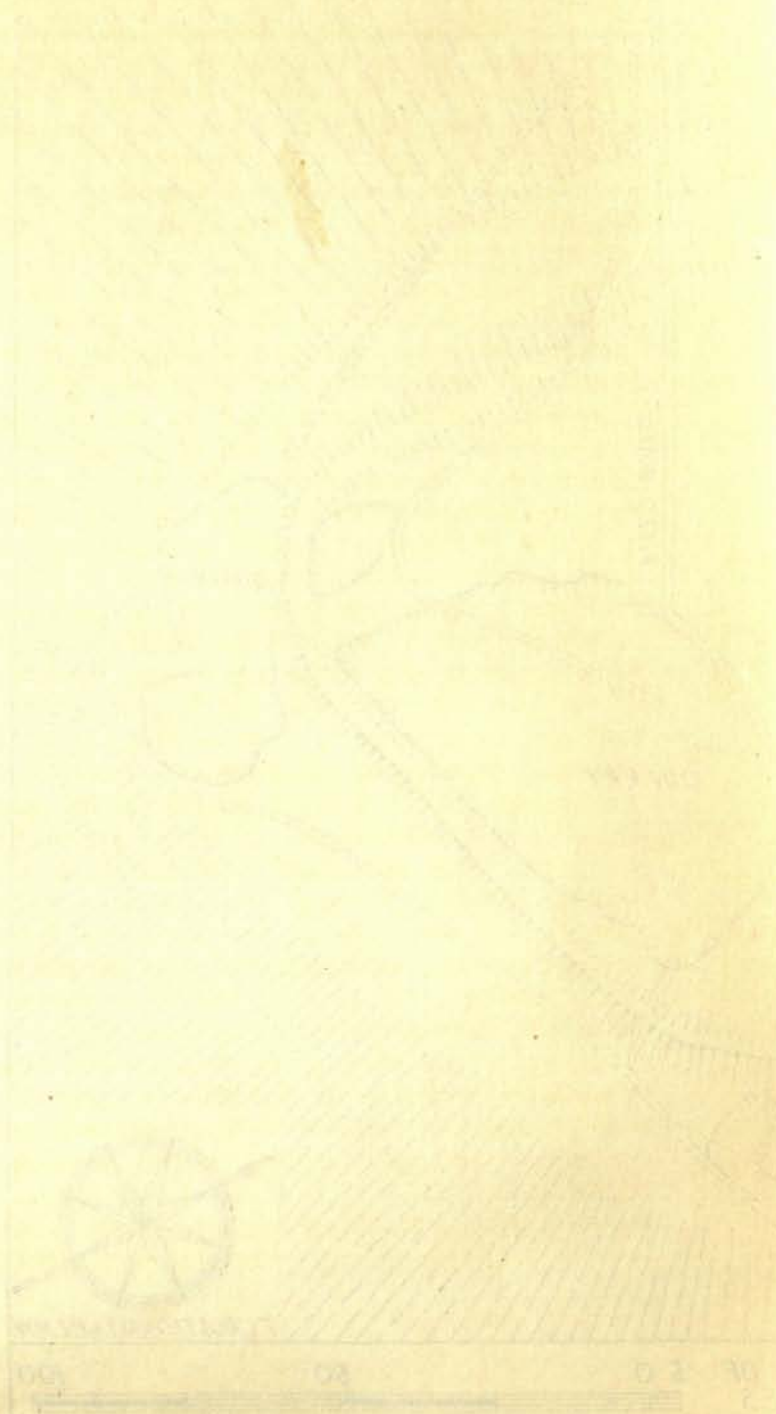


FIG. 7 (see p. 72)



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use of supplementary lines of rampart and ditch for some part of their defensive systems, but it is geographically difficult to link them up directly with the remote regions of southwestern Britain. Moreover, in La Tène III the Seine valley was Belgic (in the narrower sense of the term), and there is nothing Belgic in the cultures associated with the Iron Age B fortifications of that period in Dorset or Cornwall. At the same time an earlier origin of 'Wessex hill-fort B' in the Seine valley is rendered equally unlikely by such a fact as the common occurrence of La Tène II brooches in Seine Inférieure and their extreme rarity in the hill-forts of the Dorset-Cornwall area. On all grounds, eastern Normandy may be excluded as the continental source of 'Wessex hill-fort B'.

In short, on structural evidence alone the main source of the British multiple earthworks, with the associated use of the sling, is now abundantly evident. It is to be found within that tract of southern Brittany which lies between the Black Mountains and the sea: in the Morbihan and the adjacent portions of Finistère and Loire Inférieure. Nor, in the light of history, is that conclusion surprising. For this part of Brittany was occupied or dominated, in and before the time of Julius Caesar, by the Veneti, the great traders of the northwest, who formed the spearpoint of the Gaulish resistance to Caesar during his final campaigns in northern France. The ships of the Veneti were large seagoing vessels, equipped with leather sails—fitting transports for precursors of the Vikings of a later age. And that the Venetian merchants were in regular contact with Britain is illustrated by the statement of Strabo that they were stimulated in their opposition to Caesar by his threatened voyage to Britain, on the ground that 'they were using the emporium there',¹³ and did not want to lose it.

Thus the region of northwestern France where alone any significant number of multiple camps occur, where alone the sling is known to have been widely used, was dominated by a notably powerful tribe having a regular commercial relationship with Britain. Our problem seems to be on the way to solution, but there are still difficulties. Whilst cliff-castles such as Castel Coz or Castel Meur are identical structurally with other cliff-castles in the Scilly Isles or Cornwall, can they be closely compared with the large multiple *oppida* of Wessex, such as Maiden Castle? Two points demand consideration in this context. First, amongst the multiple camps of the Venetic area are some of considerable size: for example, the camp on the Pointe de Penchâteau,

¹³ *Geog.*, IV, 4, 1.

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Pouliguen (Loire Inférieure) is some 20 acres in extent, and that on the Pointe du Vieux Château on Belle-Ile-en-Mer (Morbihan) is as much as 100 acres. Secondly, in considering the large multiple camps of southwestern Britain, it must be recalled that some—perhaps many—owe their origin and their size, not to the Iron Age B people who eventually controlled them, but to the preceding Iron Age A people. Thus Maiden Castle had already reached its maximum extent, within the defences, during its occupation by the Iron Age A people in the third and second centuries B.C. When the Iron Age B people arrived in the first century B.C., they merely assumed control of the fully grown camp, with its single rampart and ditch, and proceeded to enlarge and multiply its defences. A similar sequence of events can be recognized at Hembury Fort in east Devon. If, therefore, these examples are typical, as we have every reason to expect them to be, the size of the large British camps should not be regarded as an intrusive feature of the Iron Age B phase, but rather as a legacy from Iron Age A; as evidence, in fact, of the extent of the existing population of western Britain at the time of the arrival of Iron Age B.

A second difficulty is at first sight more serious. The transference of a new type of warfare, and a new type of defensive armament from one side of the channel to the other might reasonably be expected to include the corresponding transference of a complete culture. Such a transference did in fact occur at the time of the migration of Belgic tribes into southeastern Britain in the first century B.C. But we look in vain in France for any group of pottery closely resembling that which characterizes the western British camps, such as Maiden Castle or Hembury, in the Iron Age B phase. Whilst certain elements of the 'Wessex hill-fort B' pottery—bead-rims, countersunk handles,¹⁴ etc.—are found here and there in northwestern France, they are always associated with other elements foreign to the British series. It is clear therefore that the multiple fortifications of western Britain were not introduced by a folk-migration from France on any considerable or comprehensive scale; and this conclusion is supported by another significant factor. From La Tène II onwards throughout northern

¹⁴ The countersunk handles which are a characteristic feature of 'Wessex hill-fort B' (FIG. 2, no. 6) occur on five sites in northern France: at Plouzevé and Huelgoat in Finistère, at S. Donan, 10 kms. s.w. of S. Brieuc in the Côtes-du-Nord, at Kerhillio near Carnac in the Morbihan, and at S. Nazaire in Loire Inférieure. Moreover, some of the pottery from Castel Coz (Finistère) shows a tendency towards the bead-rim, not incomparable with that of the B pottery of southwestern Britain.

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France the use of the potter's wheel, though not invariable, seems to have been normal. In Britain it first appears in the southeast with the arrival of the Belgic immigrants of the first century B.C., and, save for a few foreign elements at Hengistbury Head (see below), it was not found in western Britain until the westward penetration of Belgic influence in the first century A.D. Prior to that event, it is nowhere a feature of the Iron Age B cultures; and it is impossible therefore to suppose that those cultures, as represented by the pottery, reflect any considerable immigration of craftsmen from any part of northern France. Gaulish potters of La Tène II-III, immigrating into western Britain in sufficient numbers to dominate the craft of their adopted country, could not have failed to introduce so easily acclimatized a device as the wheel with the same facility shown by their contemporaries, the Belgae, in eastern Britain.

It is clear therefore, that the immigrants who brought from southern Brittany into southwestern Britain the sling and the multiple earthwork, were not accompanied by any large train of potters. At the same time, as we have seen, their arrival (at any rate in Wessex) coincided with the gradual introduction of new ceramic types, amongst which the bead-rim is the dominant feature. What is the explanation? It can scarcely be other than this: the bead-rim, as observed above, is essentially a trick of the metal-worker to reinforce the rim of a metal pot, and the handmade bead-rim pots of the Wessex camps (FIG. 2, nos. 4-6) may therefore be regarded as copies, made mostly by the Iron Age A potters, of the metal vessels introduced by their new masters, the Iron Age B invaders. If the question be asked, where are these metal prototypes?—it is probable that the answer has already in effect been given: namely, that like other metalwork they have perished in the acid soils on which most of the camps are situated. But in fact occasional examples of the bead-rim bowl of metal have been preserved; notably, the bronze bowls from Spettisbury Camp, Dorset (British Museum), and Glastonbury lake-village (Taunton Museum). The Spettisbury bowl exhibits all the essential features of the bead-rim pots of Maiden Castle, and the Glastonbury bowl shows the multiple bead-rim which is a variant in the same ceramic series. Both bowls show an experienced craftsmanship which implies the former existence of many of their kind.

Let us now take stock of the problem as a whole. The remodelling and reinforcement of earthworks such as Maiden Castle in the first century B.C. was the work of members of the tribe of the Veneti from southern Brittany, who arrived in small numbers with no considerable

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train of craftsmen (or craftswomen) but obtained complete dominion over the large but unenterprising population of the Wessex downs. Under the new (Iron Age B) régime, the bulk of the population of *oppida* such as Maiden Castle remained of the old stock; but their potters, with the usual imitativeness of the craft, adapted the forms of the metal vessels and, to some extent, of the pottery vessels which accompanied the invader. The result was a typical example of British compromise: new elements were absorbed gradually into the corpus of civic culture without any very violent essential change. Only in the actual mechanism of warfare and defence had the change been instant and drastic—the reflection of the act of military domination with which the new régime had come into being.

From these reasoned inferences it is both easy and perilous to embark upon a sea of conjecture in the search for historical contexts. What were the motives for the implanting of strong and determined military minorities from Venetic Brittany upon the teeming agricultural population of the Dorset downs? Not trade at any rate—that Veneto-British trade to which Strabo refers in the passage already cited. For, if once again Maiden Castle may be taken as typical, trade entered scarcely at all into the economy of the Wessex downland communities. Practically everything used in *oppida* such as Maiden Castle and Hembury was of local origin. The urban peasantry which these sites represent fulfilled almost all their needs from their environment, and had at the same time little or nothing of value to export. In good years there may have been a little surplus on the corn-crop, but, though corn is included by Strabo amongst the British exports, he may be supposed to have had in mind the progressive Belgic settlers of south-eastern Britain rather than the conservative peasant-farmers of the west. In Wessex at least, the foreigners from southern Brittany arrived as settlers, not as traders.

Further west, in Cornwall, the case may have been different. There, cliff-castles identical with those of southern Brittany occur in the vicinity of negotiable ores, and there if anywhere may be recognized the primary colonies of the Venetic traders. Unfortunately little is at present known of the culture represented by these Cornish cliff-castles, and further exploration in them is urgently needed. In the meantime it is fair to infer that the multiple earthworks of Cornwall represent Venetic trade, whilst those of Wessex represent Venetic immigration; and that the former at least were the product of Venetic activity before the time of Julius Caesar.

IRON AGE CAMPS IN FRANCE AND BRITAIN

This brings us to the general consideration of date. The 'Wessex hill-fort B' culture—*i.e.* the culture associated with the multiple earthworks of a site such as Maiden Castle—is tied chronologically at one end by the fact that in its latest phase it is incorporated in the Belgio-Roman culture of *c.* A.D. 25–65. Moreover, the associated metal-forms are all of La Tène III; and taking into consideration the general course of its development in Wessex, no archaeologist is now likely to dispute the view that it arrived sometime during the first half of the first century B.C., *i.e.* at about the time when Belgic tribes, bearing a different culture, were settling further east, in Kent, Essex and Hertfordshire.

From this premise, it is tempting to go further and to speculate upon the historical circumstance of this extension of Venetic authority to Wessex. In doing so, I lay no emphasis upon the suggestion which on general grounds seems to me most likely but cannot at present be proved. Trade may be supposed to have preceded settlement, and it may be assumed provisionally that the Venetic settlements in the vicinity of the negotiable ores of Cornwall are earlier than the period of Venetic domination over the urban peasantry of Wessex. For the latter, representing, as it does, migration and permanent settlement, the history of northern France in the appropriate period offers two possible moments. The first is that of the Cimbric invasion at the end of the second century B.C.; the second is the Caesarian invasion half a century later. Whether the former made itself so felt along the remote cliffs of southern Brittany as to compel the emigration of Venetic elements strong enough to dominate the great hill-forts of Wessex seems on general grounds doubtful. Nor perhaps is it likely that a tribe which in 56 B.C. was able to play the leading role in the resistance to the Roman invasion of northwestern France had lost a considerable part of its best blood only a generation previously. On the whole, therefore, I incline provisionally to the Caesarian invasion of 56 B.C. as the main motive.

The details of the Venetic resistance in that year are familiar. The tribesmen had already sent across the Channel to Britain for help¹⁵—an action consistent alike with the pre-existence of Venetic settlers on our shores and with a subsequent recourse to Britain by Venetic refugees. Such was the resistance of the Veneti and their allies, and so nearly did they prevail, that Caesar, after the great naval battle off

¹⁵ *Bell. Gall.* III, 16.

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the Morbihan coast, determined to teach the whole Gallic people, by a terrible lesson, that it was dangerous to oppose him. As the Venetic senate were responsible for the 'outrage' which led to the war, every man of them was put to death; and all the rest of the tribe whom he could seize were sold into slavery. It is only reasonable to suppose that in fact members of the tribe did escape and that amongst them were chieftains who might lead the remnant of the tribe to new homes overseas. In such a débacle, the older men and the women and children are likely to have been the most numerous victims, and the slaughter or enslavement of the women would remove a large proportion of the potters from the tribe—a likelihood consistent with the absence of any numerous train of potters from the refugee-immigrants who reached Wessex.

Here then is an obvious motive for the settlement in southwestern Britain, not of prospectors searching for ore but of fugitive princes with a scanty following in search of new homes and fresh power in a country already partially colonized by their own folk—a motive, in fact, for the Venetic domination of the large but culturally decadent urban peasantry of the Wessex downs, to the east of the area of primary Venetic settlement. If this hypothesis is found to be consistent with future evidence, we have then an important new fixed date in British history—the year 56 B.C. for the introduction of Wessex 'hill-fort B'. If we remember that, with some Belgic dilution, this culture continued until after the middle of the first century A.D., no theoretical difficulty arises on grounds of typological development; and, without premature emphasis, I am inclined to regard 56 B.C. as a likely initial date for the Venetic or Iron Age B conquest of the Wessex hill-forts. Others may prefer to bring Venetic merchant-princes to the agricultural milieu of Wessex whilst their Gallic homeland was still intact and unconquered, and to leave the context of their migration an unsolved problem. In point of time, the difference would in any case amount to no more than a decade or two, and this alternative view would not affect the basic results of our survey.

POSTSCRIPT.—No attempt is here made to illustrate the pottery and other finds from the camps above described, with the exception of the two dominant types from Le Petit Celland (FIG. 8). Of these types, no. 1 is a finely made cordoned bowl of dark grey or black ware, and is identical with the cordoned bowls of Hengistbury class B (J. P. Bushe-Fox, *Hengistbury Report*, 1915, pl. xvii, 3). This pins down the date of the Hengistbury type to the first half of the first century B.C., although coarse derivative forms are occasionally incorporated in the western Belgic culture of Britain in the first

IRON AGE CAMPS IN FRANCE AND BRITAIN

century A.D. ; *e.g.*, Bulleid and Gray, *Glastonbury Lake Village*, II, pl. LXXX, 190, and an unpublished example at Maiden Castle in a layer dating from the eve of the Claudian invasion. At Hengistbury Head these cordoned bowls are wheel-made and may safely be regarded as direct importations from across the Channel. In this connexion it may

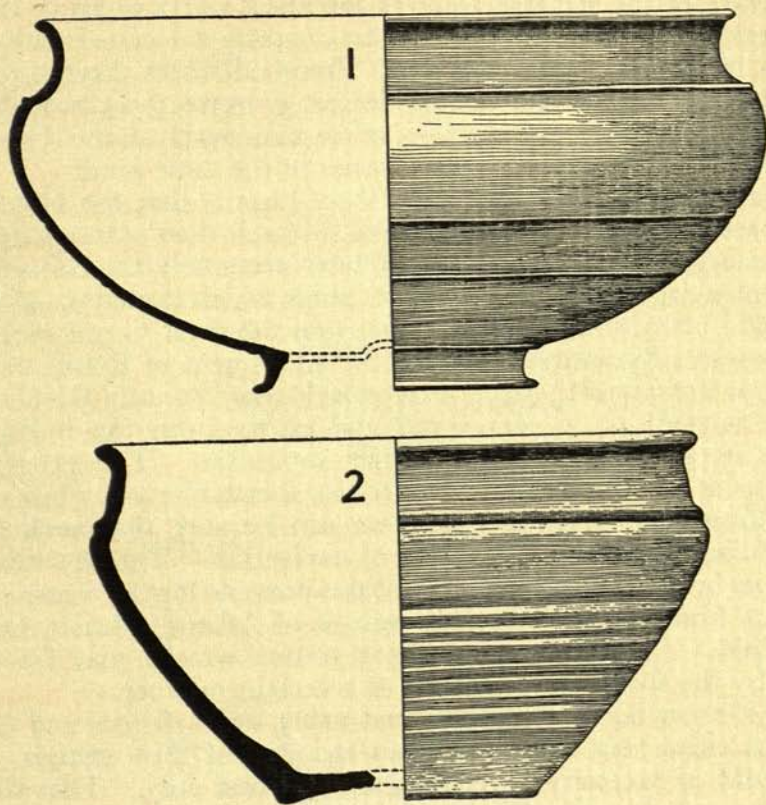


FIG. 8. TYPICAL POTTERY FROM LE PETIT CELLAND 56 B.C. (1)

be remarked that Hengistbury Head lies immediately opposite the Manche, the Department in which Le Petit Celland is situated ; and, although there is a danger of overworking the refugee-explanation, it is by no means unlikely that the Hengistbury bowls are in fact the relics of a few shiploads of refugees at the time of the Caesarian conquest of northern Gaul. This explanation is supported by the absence of the Hengistbury-Le Petit Celland type elsewhere in Britain.

The same remarks apply to the graphite-coated pottery of Hengistbury Class H, which is contemporary with Class B and is likewise an importation from Brittany or western Normandy (Le Petit Celland, Kercaradec, etc.).

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SUBMARINE RESEARCH IN GREECE (PLATES I-IV)

In view of the fact that some of the finest works of art of antiquity in existence have been found in the sea, usually associated with wrecks of ships in Greek waters, and since almost all these discoveries have been made by chance and not by design, it seems most probable that a properly organised examination of certain parts of the Greek seaboard would result in further discoveries of the same kind.

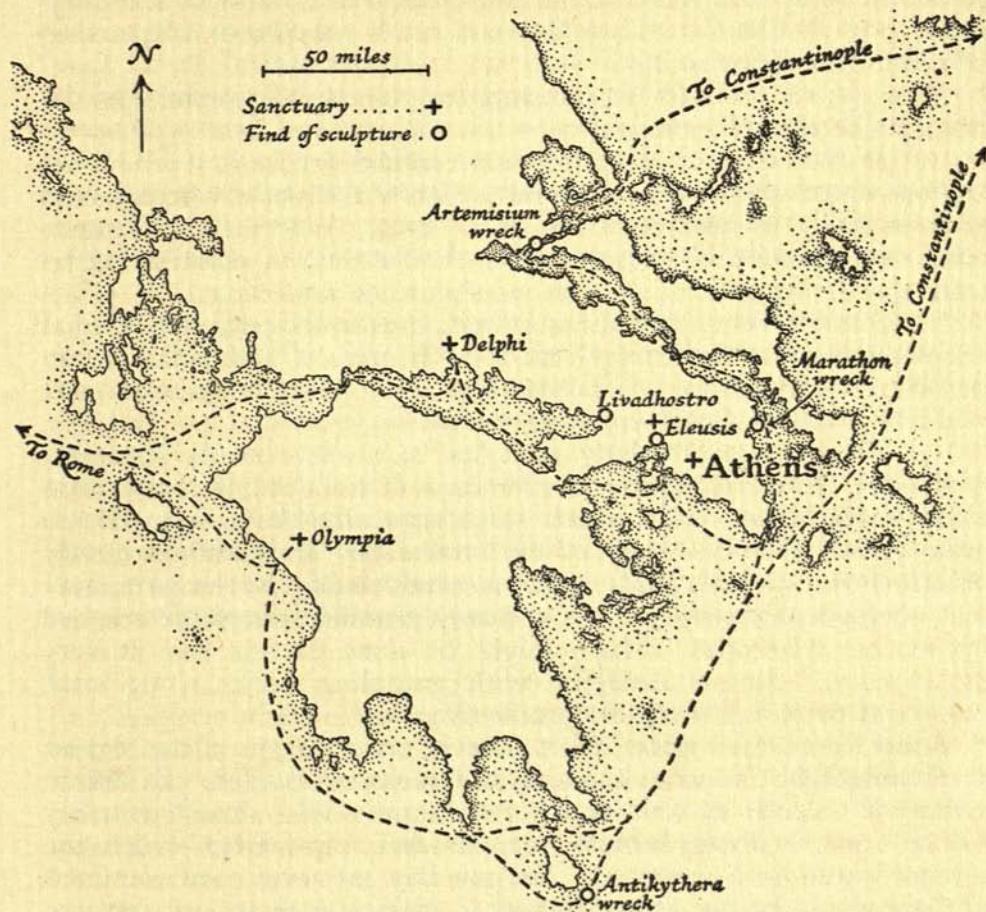
The advantage of research on these lines is that the Greek coast is more suitable for careful submarine research than other parts of the Mediterranean, since we know here fairly accurately the courses taken by ancient navigation. The routes of ships which loaded up cargoes of statues and other antiquities of value from the great Greek sanctuaries in Roman and Byzantine times for the adornment of Rome and Constantinople are known, partly from the literary records which tell us how the ancient trade routes ran and partly from the indisputable evidence of actual wrecks containing antiquities. The geographical limitations of Greece make certain routes inevitable, and when we find wrecks strewn along those routes we can be sure that such ancient cargoes followed certain fixed lines of navigation. The accompanying map shows by dotted lines the only routes possible for the transportation of cargoes from the great artistic centres of Athens, Eleusis, Olympia and Delphi. Anywhere along these routes wrecks may be found, judging by the fortunate discovery of a certain number.

But it must not be forgotten that many small shrines and temples as well as cities may also have been stripped of their statues. Such cities would of necessity have been on the coast only. That this was done is suggested by the discovery of two bronze statues, one at Eleusis and one in the remote Boeotian bay of Livadhostro, which were in the sea at no great depth. These and the various wrecks will be dealt with *seriatim* below.

Statues of marble when immersed in the sea for any length of time usually corrode or are damaged by barnacles. But statues of bronze not only do not corrode but preserve their original surface almost unimpaired even when immersed in sea-water for over two thousand years. The Zeus of Artemisium and the Marathon Boy, or the Antikythera Athlete (see below), rank among the best preserved bronze statues in existence. This is the more important in view of the fact

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that in the fifth and early fourth centuries B.C. the great majority of masterpieces were made of bronze and not of marble. Bronze, on the other hand, corrodes rapidly in the earth and very few well-preserved bronzes have been found in excavation compared with the



MAP SHOWING ROUTES OF SHIPPING AND SITES OF WRECKS

number of those found in the sea. Submarine search, therefore, for cargoes of works of art would be likely to recover bronze statues of the best periods by the most famous artists. In the case of small shrines and temples or small provincial cities near the sea there is another alternative possibility of research. At places like Livadhostro or

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Eleusis or Sunium, where the shrines abutted close on the shore, it may well be that looters and enemies at various times have overturned the temples and sacked the buildings. Statues would be cast out and would tend to fall over into the sea. This, in my opinion, explains the discovery of the bronze statue of Poseidon at Livadhostro and the bronze boy at Eleusis. They did not reach the places where they were found by shipwreck.

The general method of procedure for submarine research should, I think, take the following form :—

1. A close analysis of statistics of recorded wrecks in recent times in Greek waters should give some indication of the places where wrecks are most likely to have occurred in all ages. Where those danger-points cover ground of no great depth under the sea examination by divers and diving bells of the sea-bed should be undertaken.

2. Since in every case the information as to the presence of what wrecks are known has come from local fishermen and peasants, an intensive enquiry among coastal dwellers in all likely neighbourhoods should be made by Greek-speaking archaeologists.

3. There are many long stretches of shallow sandy coastline, like the bay of Marathon, the interior coast of Euboea and the western Peloponnesian coast, upon which ships may often have been driven during gales. The discovery of the bronze boy at Marathon clearly indicates a wreck of this kind. Here research could take the form of an aerial photographic survey or of a steady examination of the sea-bed with marine telescopes. Much might be done in this way at very little expense. Simple dredging with grappling hooks along such stretches of coast might also be productive.

Apart from actual works of art as such, many objects of the highest interest might be found at the sites of famous sea-battles. Diving in the Bay of Salamis as well as at Artemisium would almost certainly produce arms, helmets, bronze weapons and ship-fittings which the sea water would have preserved. Salamis Bay has never been examined and there would be but little difficulty in making such an examination.

I here summarize the information about the actual discoveries mentioned above :—

ARTEMISIUM

In 1926 some fishermen brought secretly to Athens the bronze arm of a large statue. They stated that it had been brought up from the sea in their nets near Cape Artemisium and offered an option on the

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remainder of the statue that it represented to various Greek and foreign dealers in Athens. No one took up the option, but the Government got to hear of it and forced the fishermen to give full information. In consequence operations were undertaken by the Government at the spot, which resulted in the discovery of a wreck from which was obtained the remainder of the bronze statue, which turned out to be the finest Greek bronze known, a statue of Zeus¹ (or Poseidon) (PLATE I) almost perfect and of great size (6 ft. 8 ins.). An almost complete statue of a small boy (PLATE II, I),² and the forequarters and part of the rest of the body of a galloping horse (PLATE III).³ The Zeus is the work of a master of the fifth century, about 460 B.C. The boy is a superb example of Hellenistic work of the late fourth century, unequalled in any museum. The horse is fourth century work also, perhaps one of a pair.

The wreck was situated 700 metres from the shore near the sandy spit where ships turn from the interior channel between Euboea and the mainland and head for the open sea. The depth of the wreck was 144 feet (24 fathoms). It is at present uncertain whether this was a shipload of works of art which was coming from the south or from the north. Pottery found in the vessel is thought to date it to the first century B.C., in which case the destination of the ship must have been Rome and the ship would therefore have been coming from the north and going south. In that case its loot would have come probably from some rich town of Thessaly, perhaps in the Gulf of Volo.

LIVADHOSTRO

A statue of Poseidon in the best style of the late sixth century B.C. was found in the sea near the village of Livadhostro at the extreme east end of the Gulf of Corinth on the Boeotian coast. It lacks only the two arms and is of a scale about two-thirds that of natural size. No accurate record of how it was found survives. (PLATE IV).

ELEUSIS

The lovely statue of a boy in bronze, now in Berlin, was found in the shallow sea off the sanctuary of Eleusis in similar circumstances by fishermen. Here also full details of the method and occasion of the discovery are lacking.

¹ See *ANTIQUITY*, 1930, IV, 412 and plate IV *facing* p. 409, illustrating the head.

² *Ibid.* p. 413.

³ *Ibid.* p. 413.

ANTIQUITY

MARATHON

In 1925 fishermen in the Bay of Marathon dragged up in their nets an almost perfect statue of a boy in the style of Praxiteles (PLATE II, 2). The statue is almost perfect and its surface-condition is superb. It comes, if not from the actual workshop of Praxiteles, at least from the hand of one of his pupils. Here no further search was carried on, but since there is neither temple nor shrine nor town in the vicinity the statue is almost certainly derived from the wreck of a ship blown by the north wind on to the shallow shoals of the Marathon shore. This coast is remarkable for the shallowness of the sea-bed, and for the heavy deposits of sand which are cast up by storms. Further search hereabouts for the remainder of this wreck is most essential. Very little effort would be needed to clear the wreck once it was found. This wreck would be comparable to that at Artemisium, further up the same channel. Both are perhaps filled with loot from northern cities after the conquest of Macedonia by Aemilius Paulus.

ANTIKYTHERA

Here in 1900 was found the most productive wreck ever discovered in Greek waters, though the quality of the works of art recovered cannot compare with that of those described above. Antikythera is a small island lying south of Kythera which itself is south of Cape Matapan. Between these two islands or between Kythera and the mainland passed all shipping, as indeed it passes today, that cannot negotiate the Isthmus of Corinth. Here at only about 180 feet (30 fathoms) depth were found, some 75 feet from the shore, numbers of marble statues and one fine bronze statue of an athlete. This ship was probably conveying loot from Athens to Rome. It is significant that we know of two other wrecks near this spot, one of a ship mentioned by Lucian which was taking to Rome a picture which Sulla had obtained at the capture of Athens, the other the brig 'Mentor' which Lord Elgin sent to England with a consignment of sculptures of the Parthenon and of other Athenian buildings. The passage round the southern capes of the Peloponnese is exceedingly dangerous. It is interesting to note that the brig 'Mentor' which sank in 1802 was completely salvaged in 1804, and all the lost sculptures were recovered (see *Journal of Hellenic Studies*, xxxvi, 259), although it is persistently believed that even the whereabouts of the wreck remain unknown and that nothing was saved (see ANTIQUITY, 1930, IV, 414).

The harbour into which the brig 'Mentor' fled for refuge in a

NOTES AND NEWS

gale is exactly the kind of place where other ships similarly placed would have taken refuge and perhaps have been wrecked in the same way. Further investigation of this harbour should be undertaken. Full records of the mishap of the 'Mentor' are preserved, with maps, in the Elgin papers (see *Journal of Hellenic Studies*, xxxvi, 242).

The whole of the western coast of the Peloponnese should be examined. So far there is no record of shipwrecks here but the coast is sandy, particularly in Messenia, and on the little islet of Proti near Kyparissia is a small harbour, the rocks of which are covered with graffiti written by sailors who fled there for refuge. These graffiti cover over two thousand years and testify to the stormy nature of the coast. Ships bound for Constantinople, loaded with loot from Olympia, might well have been wrecked here in trying to make the harbour. In any case the whole coast is unexamined and its proximity to Olympia would make it worth while.

The whole coastline of the island of Kythera (or Cerigo) should be looked at. The exterior coastline of the island of Euboea, notoriously dangerous in antiquity, might contain wrecks of ships which had set out for Constantinople and which had taken the direct route round the southern end of Euboea. A north wind would tend to drive them back on to Euboea.

There remains also the question of the dredging of harbours near ancient sites. The astonishing discovery in 1931 of a large group of Greco-Roman reliefs of very great beauty and importance, of marble, in the mud of the harbour of Piraeus at Athens shows how much there may be to be found. In this instance the mud had acted as a preservative from the corrosive action of the sea-water and the reliefs were in consequence in very fine preservation. But harbours heavily silted up with mud are rare in Greece and not equal good fortune could be expected in every harbour. But bronze objects might equally be found. Another similar discovery on a smaller scale is that of a fine fifth-century relief in the harbour of Sozopolis on the Bulgarian shore of the Black Sea near the site of the famous city of Apollonia. Here also further research might be fruitful. Apollonia was famous for its colossal statue of Apollo in bronze by Kalamis, the Athenian master of the early fifth century. The city had paid some £125,000 for this statue, the largest recorded payment ever made to an artist in antiquity. No trace of the statue has ever been found and it was known to have stood on an island in the harbour. Pliny asserts that it was taken to

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Rome. This may be true, but there must have been other statues on the island.

Another harbour worth examination is that of Cnidos. The world-famous Aphrodite of Praxiteles stood near the harbour. It was destroyed by the early Christians and its fragments might be recovered from the harbour sand.

In general, harbour-dredging would seem a most profitable undertaking, to judge by the recorded chance discoveries and considering the historical information to which we have access.

NOTE. Reference may also be made to an article on 'Submarine Discoveries in the Mediterranean' by Monsieur A. Merlin, Membre de l'Institut de France, Conservateur au Musée du Louvre; it was published in ANTIQUITY (December 1930) IV, 405-414, with four plates.
S. CASSON.

PHOENICIAN CARRYING TRADE, SYRIA (PLATE V)

In June 1938, while travelling along the coast road from Ras Shamra to Beirut I noticed a consignment of some hundreds of baked clay amphorae neatly stacked in rows, on the foreshore of a small Syrian beach called el Böss, 5 km. south of Baniyas and 55 km. south of Latakia. PLATE V shows a part of the cargo ready for distribution. On making enquiries from the Arab sailors whose ships (*shakhtur*) were at anchor, I extracted the following information.

The pots are made in Saida (ancient Sidon) whence they are exported by sea to el Böss, a journey of about 240 km. (150 miles). They are exchanged against donkey loads of sheep-dung which are brought down to the coast by peasants from the Alaouite hills. This is a perfect example of commercial barter, for there is no money transaction whatsoever. The dung is afterwards shipped to various coastal towns, in particular to Tripoli where it is in great demand as a fertilizer for orchards and gardens. The sailors told me that on occasion their ships sail as far as Palestine.

The largest ships (*ghiz*) are said to be able to carry a cargo of 1000-1500 pots, the smaller ships (*kik*) or caiques carry from 200-400.

The equations involved are as follows: one donkey carries one sack of dung (*zibl*); one sack of dung is exchanged against two amphorae (*jarrat*); one amphora, measuring about 70 cm. in height, costs about 6 piastres and this at the present rate of exchange (876 piastres to £1 sterling) is a fraction over 1½d. The total value of the biggest cargo is therefore a little over £50 sterling.

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The shippers have an agent who keeps a small tavern close to the sea-shore and he warns the peasants of the approaching arrival of the ships. The peasants then spend several days collecting the dung which is ready when the cargo lands.

The names of two of the sea-captains (*rais*) were Adamkōk, a strange and to me new Moslem name, not met with in the Hinterland of Syria and Ali Hajuz from Banias.

An interesting point is that some of the ships come from the isle of Ruad, opposite Tartus, the ancient Phoenician Arvad, whence they proceed to Phoenician Sidon.

Here therefore we have a remarkable relic of the ancient Phoenician carrying-trade which the mariners themselves admit to be hereditary, plying between the ancient Phoenician sea-ports and carrying a type of vase used by the ancient Phoenicians themselves.* This gives us an insight into the ancient methods of carrying and trading pottery. It must be noted that the potters themselves do not travel on board.

Those interested in the subject should consult the stimulating account of the modern pottery trade of the Aegean by Mr S. Casson in *ANTIQUITY*, December 1938. It was originally my intention to supply him with these notes but unfortunately on consulting them I found them incomplete and returned to the same beach in December 1938 to verify certain details.

M. E. L. MALLOWAN.

THE GOGMAGOG GIANT OF CAMBRIDGE

Mr Stuart Piggott (*ANTIQUITY*, 1938, XII, 326; cp. also 1937, XI, 104) supports very strongly my suggestion (*Proc. Cambridge Ant. Soc.* 1937, XXXVII, 55-59, 64 note XII), that a pre-Christian and even pre-Roman giant-cult was associated with the Gogmagog hills near

*L. Klebs, *Die Reliefs und Malereien des Neuen Reiches*, abb. 144, XVIII-XX Dyn. c. 1580-1100 B.C. for a picture of a Phoenician ship with amphorae. Also, *Montet, Byblos et l'Egypte*, pl. CXVI for illustrations of Phoenician amphorae from Byblos. See also the account of the ancient Phoenician wine trade in Herodotus (III, 6), who relates that the wine jars exported from Syria to Egypt were emptied of their wine and re-exported full of water—*τοὺς δὲ ἐκ Μέμφιος ἐς ταῦτα δὴ τὰ ἀνδρα τῆς Συρίας κομίζεον πλήσαντας ὕδατος*. I am indebted to Mr R. D. Barnett of the Department of Egyptian and Assyrian Antiquities of the British Museum for pointing out these references to me.

For the use of dung as manure in antiquity cf. Plato, *Prot.* 334 A and Homer, *Od.* XVII, 296-300. 'But now he lay derelict and masterless on the dung heap before the gates, on the deep bed of mule droppings and cow dung which collected there till Odysseus had time to carry it off for manuring his broad acres. So lay Argos the hound, all shivering with dog ticks': T. E. Lawrence's translation.

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Cambridge. He doubts, on the other hand, whether there was an ancient hill-figure of the type of the Cerne giant cut in the hill-side of the Gogmagogs. It is true that John Layer who died in 1640 supposed that this figure, which survived up to the 18th century, was originally the work of undergraduates of Cambridge; but John Layer, a comparatively modern antiquary, cannot be cited as reliable evidence for this fact, because the figure was already known to an Elizabethan author, Joseph Hall, who matriculated at Cambridge in 1589 and mentioned the giant in a book which he wrote in his younger days before c. 1605.

Cp. *Mercurius Britannicus* (Bishop Joseph Hall), *Mundus alter et idem* (ed. Frankfurt, c. 1605) lib. 1, chap. 11: *Quam ferunt olim struxisse Omasium quendam gigantem, vastae molis, parem, non illi, cuius effigiem nostrates Academici e vicino colle excisam invisunt et admirantur*, etc. Cp. in addition the contemporary translation of Bishop Hall's book by John Healey, another Cambridge scholar: *The Discovery of a New World, Written Originally in Latin by Joseph Hall, c. 1605; Englished by John Healey, c. 1609; edited by Huntington Brown* (Cambridge, Mass., 1937) p. 39: '... who was of an incredible height of body; not like him whose picture the Schollers of Cambridge goe to see at Gogmagog hills'. For the biography of Bishop Hall and John Healey see the introduction to Brown's edition.

John Layer's statement, even if it should not originate from a mistranslation of Bishop Hall's *nostrates Academici . . . excisam*, has no more value to us than the many other rationalistic interpretations of facts of archaeology and folklore in which the scholars of the 17th century found extreme pleasure, and we have, therefore, no reason at all to follow his opinion. Whether the hill-figure of Cambridge was actually a pre-Elizabethan monument or not cannot be proved nor refuted with certainty. Only excavations or unpublished and unknown documents could shed new light on this most interesting problem.

F. M. HEICHELHEIM.

TOOL MARKS, TELL ED-DUWEIR, PALESTINE (PLATE VI)

The whole area below the northeast corner of the mound at Tell ed-Duweir is honeycombed with natural caves in the soft chalk, and these have been artificially enlarged at various times to make dwelling and burial places. The cave shown in the photograph (PLATE VI) is a fine example of Early Bronze Age excavation, and like others of the same period it has been enlarged from time to time by the cutting of new bays in a haphazard manner. The tool marks on the roof and upper

PLATE I



GREEK BRONZE STATUE OF ZEUS (or POSEIDON), c. 460 B.C. FROM ARTEMISIUM (see p. 83)



(2) THE MARATHON BOY (see p. 84)



(1) A SMALL BOY, FROM ARTEMISIUM (see p. 83)

PLATE III



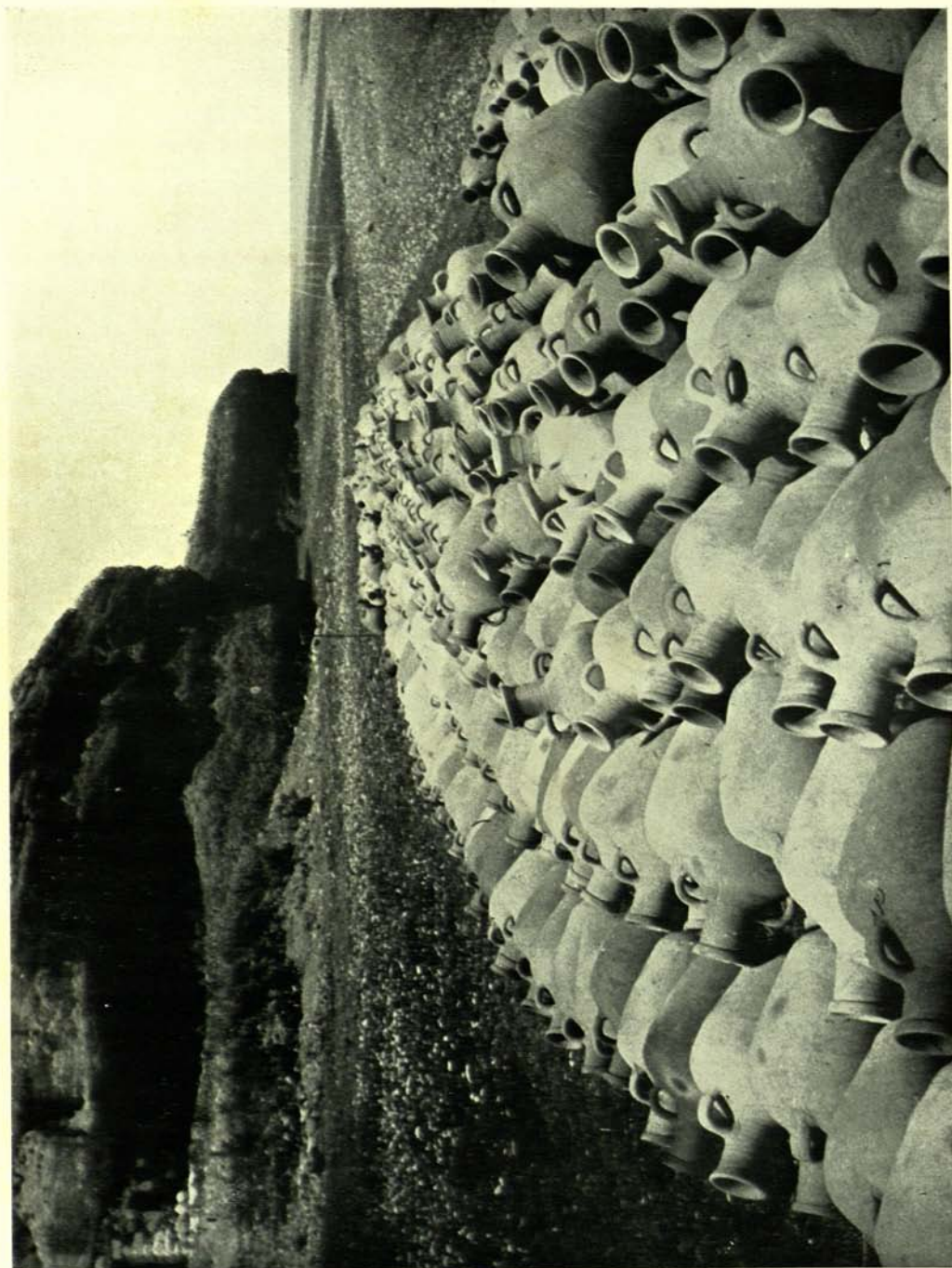
FOREQUARTERS OF A GALLOPING HORSE, FROM ARTEMISIUM (*see* p. 83)

PLATE IV



BRONZE POSEIDON FROM LIVADHOSTRO, GULF OF CORINTH (*see p. 83*)

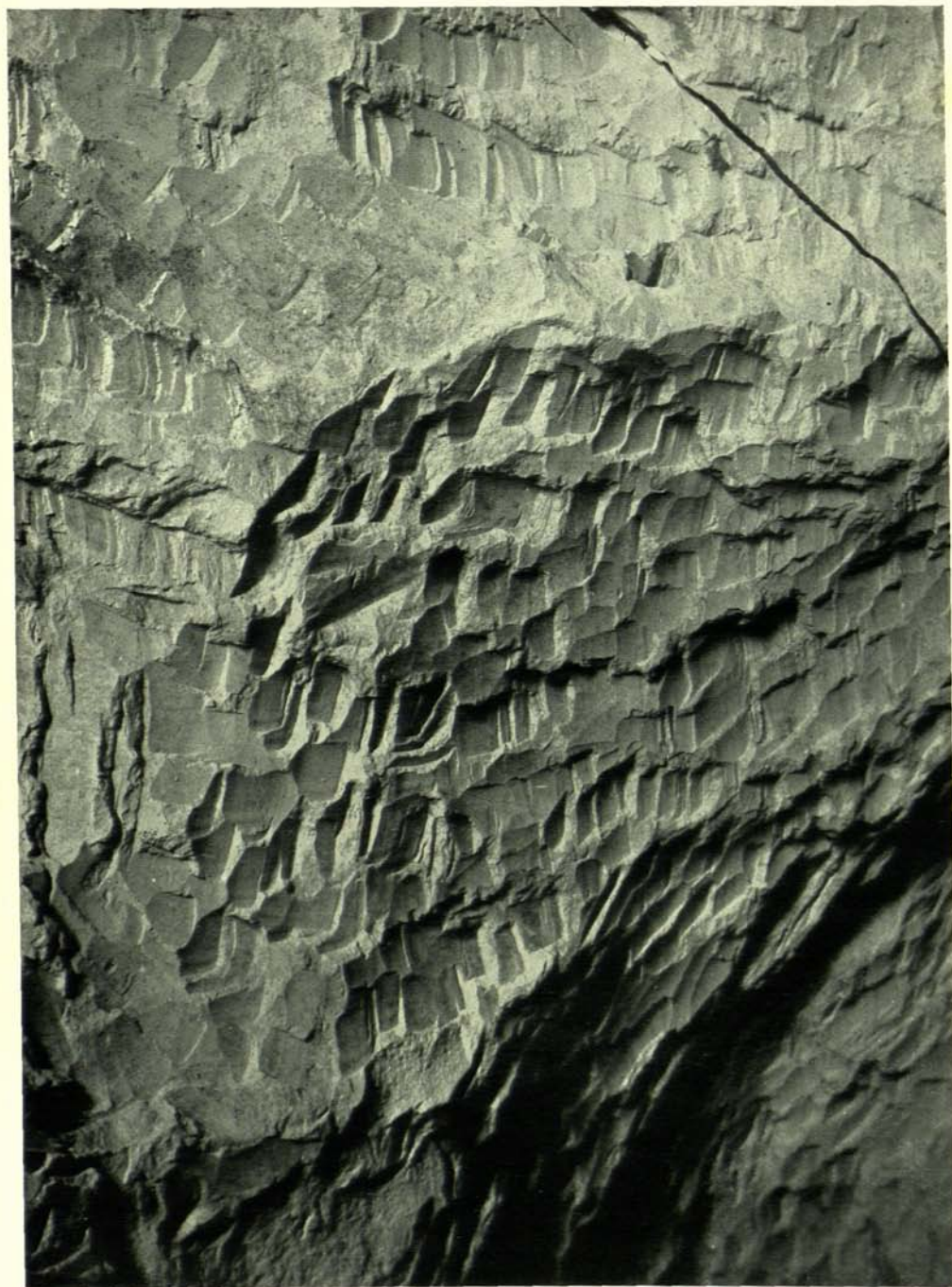
PLATE V



CONSIGNMENT OF AMPHORAE, PART OF CARGO DESTINED FOR THE PORT OF EL BOSS, SYRIA, IN
EXCHANGE FOR SHEEP-DUNG (see p. 86)

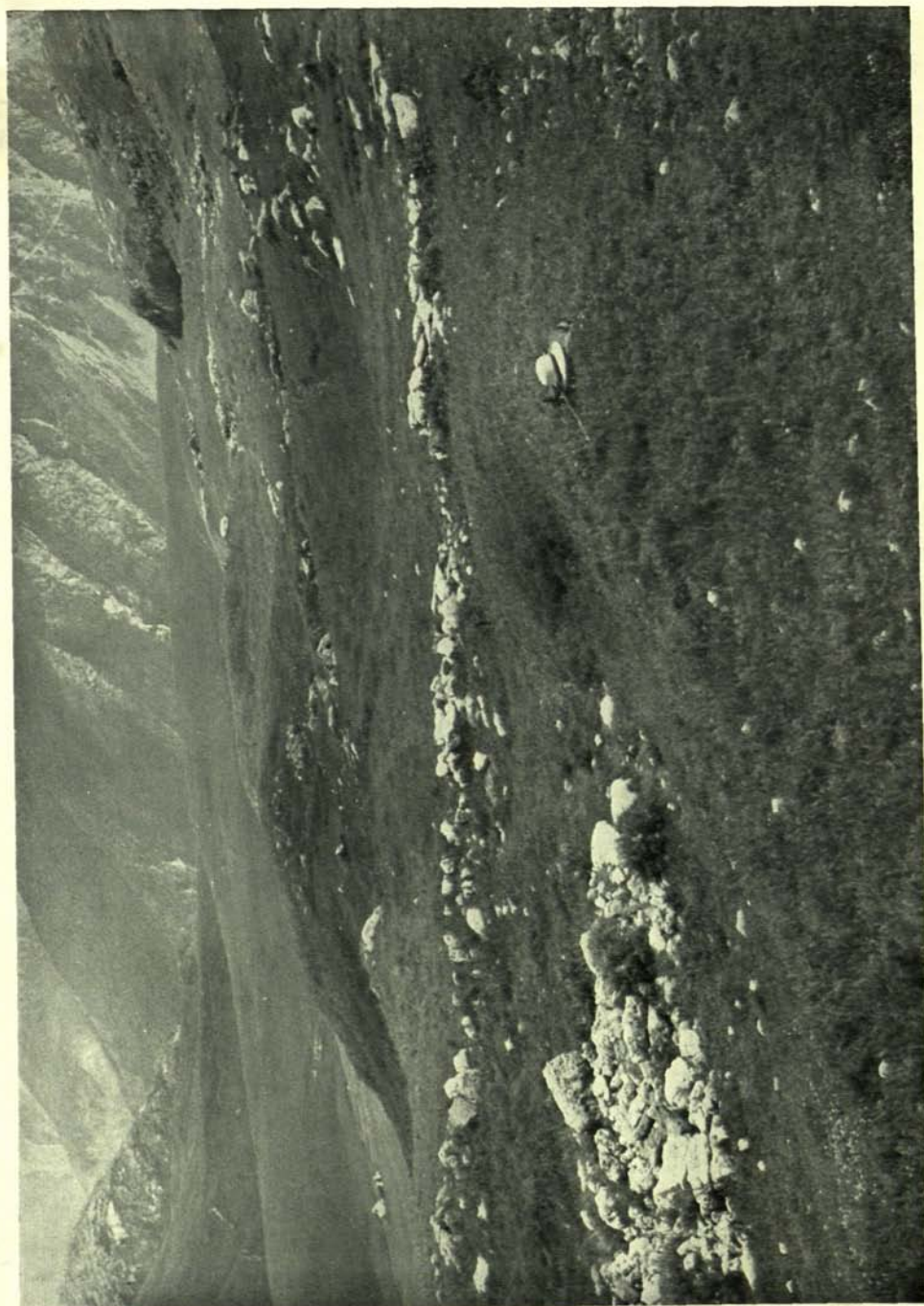
Ph. M. E. L. Mallowan

PLATE VI



TOOL MARKS IN CAVE, TELL ED-DUWEIR, PALESTINE (see p. 88)

PLATE VII

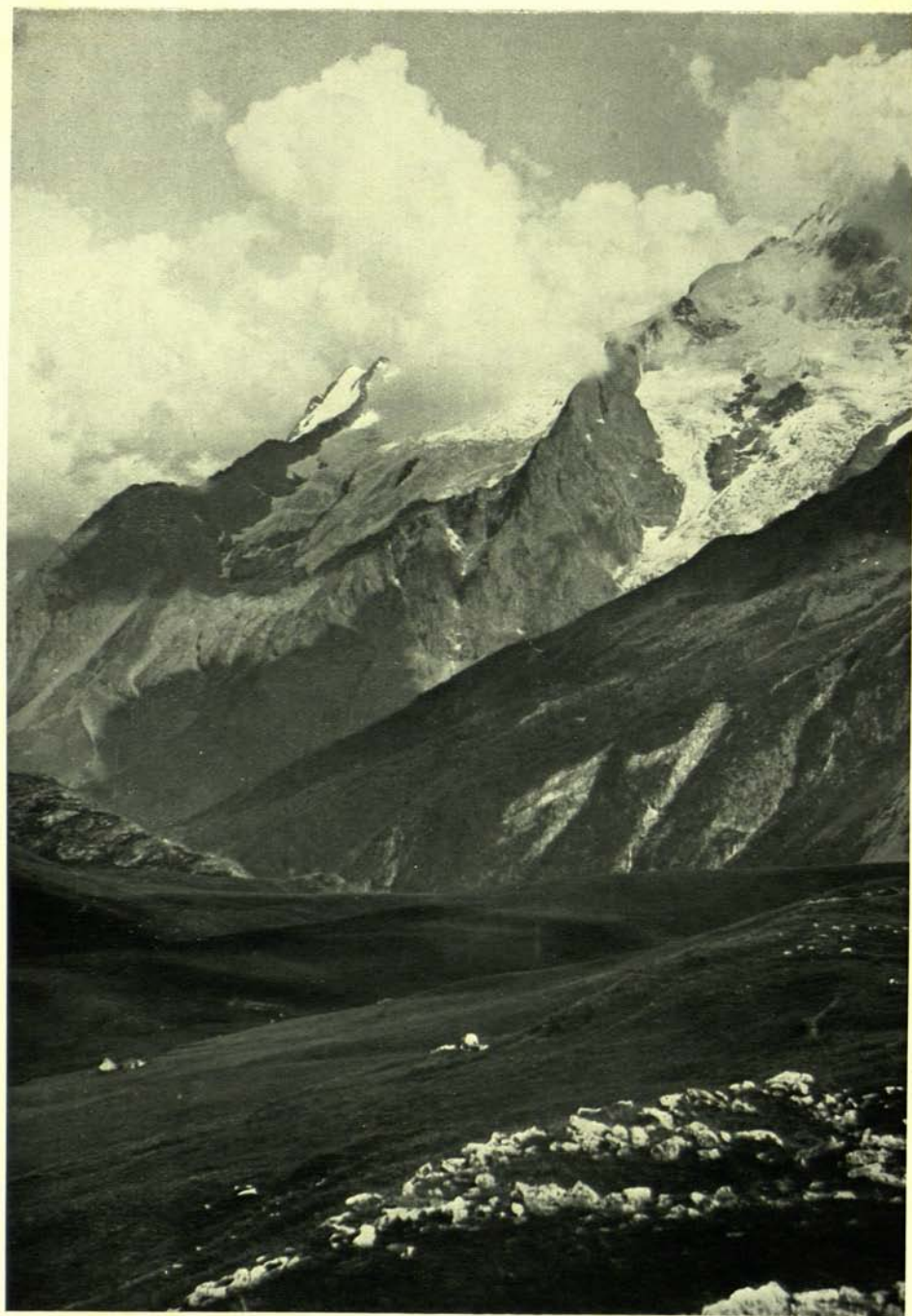


DWELLING SITES, HAUTES ALPES

Upper end of the platform, with ruins of chamber D and the crescent bank (marked by the hat) ; at the far end and in the distance are walls of paddocks (see p. 89)

Ph. W. J. Hemp

PLATE VIII



DWELLING SITES, HAUTES ALPES

Lower projecting end of the platform with chambers B and C and paddock wall. In the middle distance is the lower end of the grazing area, bounded by the Valley of the Romanche. The summit of La Meije is hidden by cloud (see p. 89)

Ph. W. J. Hemp

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parts of the walls are as clear today as if they had been newly cut, and although the cave was certainly used for domestic purposes, there is no sign of discoloration by smoke. The greatest length of the cave was about fifty feet, and the height averaged about eight feet.

The tool used had a cutting edge of $1\frac{1}{2}$ inches. It is suggested that perhaps a stone adze was used, because the blade was not sharp as a metal tool would be, but slightly rounded. In form it may have been similar to those published in *Desert Fayum* plate ix, 8 and plate xxiii, and in *Bethpelet* II, plate xxvii, 76, 77. These latter were entirely polished, and some fragments of polished adzes were found at Tell ed-Duweir and are typically Early Bronze, although none were found in the cave in question.

The chalk is extremely soft when damp, and so finely grained that it has preserved the marks of flaws in the blades of individual tools. The good preservation of the marks is due to the fact that the cave was never entirely filled with soil, though mud had percolated into the lower parts. Only two whole pots were found, one a hand-made cooking pot with rounded base and two small handles, and the other a magnificent 'holemouth' jar standing some twenty inches high. The latter was actually standing on the rock floor of the cave, covered by a large sherd which served as its lid, and the former was resting on top of the mud, as if it had floated up. There were, however, quantities of sherds, including many 'ledge' handles, jars of thick combed ware, some with incised decoration round the neck, fragments of small bowls, some burnished or with red wash, and an interesting series of pot-marks. Some sickle flints were found, including a blade $6\frac{1}{2}$ inches long.

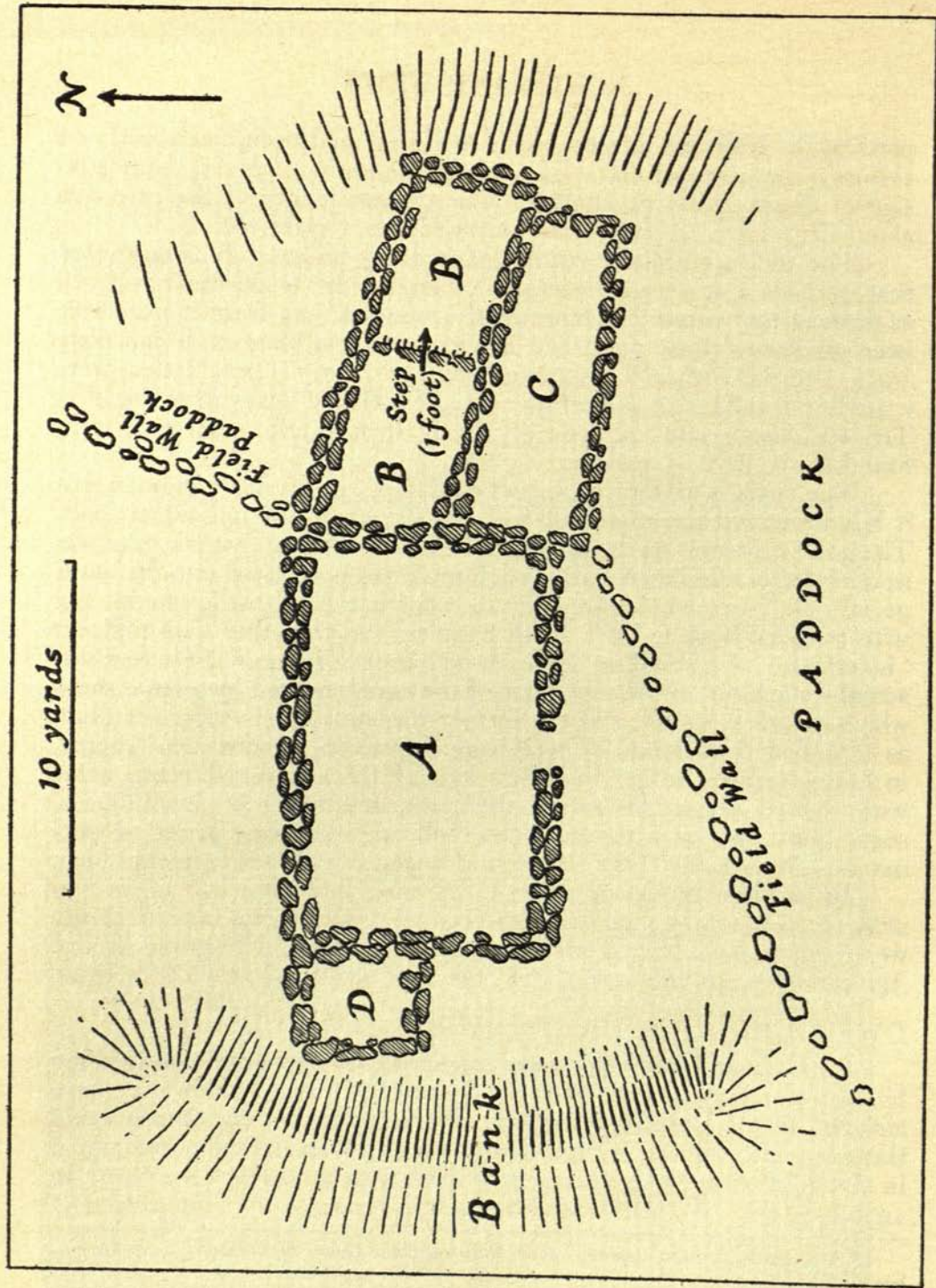
In its latest period of use, a few burials had been laid in, mostly close to the doorway; no objects were found with them, but the bodies were lying in a contracted position, which suggests a date in the Bronze Age for the abandonment of the cave.

C. H. INGE.

DWELLING-SITES, HAUTES ALPES (PLATES VII, VIII)

Sir Cyril and Lady Fox have called attention to a type of dwelling hitherto unrecognized in Wales, or indeed in Britain, which they have assigned to the Dark Ages between the departure of the Romans and the Norman conquest. Typical sites were fully described and planned in *ANTIQUITY* in 1934 (p. 402), and in *Archaeologia Cambrensis* in 1936 (p. 247). A further excavation this year, as yet unpublished,*

* A preliminary note appears in the *Bulletin of the Board of Celtic Studies*, November 1938.



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establishes a *terminus ad quem* in the Middle Ages. So far as I am aware no other examples from Britain have been published, but I have recently seen a number of sites of 'long houses' in the hills of North Wales (Carnarvon and Merioneth) which closely resemble them, although the buildings which occupied them must have been smaller. I recently found a much closer parallel—most unexpectedly—in the French Department of the Hautes Alpes at a height of about 6500 feet. The site is near La Grave, and is to be found at the eastern end of the Plateau d'en Paris, on the upper western slope of the Rif de Caturgeas, 3.20 kilometres due west of Le Chazelet village and 7.50 kilometres from the summit of La Meije (3983 m.), from which it is separated by the deep valley of the Romanche river, known as the Combe de Malaval.

Here the outstanding features of the South Wales sites are precisely reproduced:—the elaborately constructed platform, partly built up and partly excavated, set at right angles to the slope of the hill-side—although level ground was available in the immediate neighbourhood—and the crescent-shaped bank at the head of the excavated end. In the Alpine site this bank is about 16 yards long and 4 yards thick. On the platform, which measures about 30 yards by 15 yards, and lies approximately east and west, are the foundations of buildings; all are considerably ruined and none need be original. The principal 'house' (A) measures 11 yards by 6 yards (internal dimensions). This is centred on the crescent mound and the upper end is 3 yards from its base. To the lower end of this is attached a second 'house', (B), about 10 yards by 3 yards, which is not quite in line with A. Halfway along it is a step down about a foot high, and its lower end is built on the projecting part of the platform. A third chamber (C), is attached to the south side of B, 9 yards long by 3 yards at its upper end, narrowing to 2 yards at the lower, and a fourth small chamber (D), has been added to the upper end of A, it measures 4 yards by 3 and partly occupies the space at the foot of the bank. Boundary walls of a series of paddocks are also attached to the buildings (see plan, p. 90). Their slight character is apparent in PLATE VII.

It seems that this and at least one other neighbouring site of similar character must have formed the summer 'shieling' or 'hafod' for the upland pasture. Ruins of two houses of apparently later date can be seen in PLATE VII at a lower level, these also are set endways on to the hill side, but the features of the projecting platform are slight or absent, and there are no crescent banks.

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It would be of much interest to know whether this type of site has been observed elsewhere in the upland regions of central Europe.

W. J. HEMP.

MONS BADONICUS AND CERDIC OF WESSEX

Geoffrey of Monmouth was a novelist, not a historian. He wrote the 'best seller' of the twelfth century. But, like any intelligent writer of historical fiction, he made use of all the historical and semi-historical material available. Internal evidence shows his use of Gildas, Bede, Nennius, William of Malmesbury, Henry of Huntingdon, Welsh genealogies, and several saints' lives.

These sources we can trace, and see how skilfully he blended them, filling the gaps with his own fertile imagination. But beyond those statements which can be traced to some known source, and those which are clearly due to Geoffrey himself, there crops up now and then a detail which suggests that he did have some other source of information. Two such items have impressed me as being worthy of serious consideration.

The first of these concerns the chronology of King Arthur's battles. As Wade-Evans has demonstrated, there are a number of references, seemingly incidental, in Geoffrey's narrative to Continental personages such as Pope Simplicius (468-483) and the Emperor Leo (457-474). These references would imply that Arthur's career belonged to the fifth century, and that his greatest victory, Mons Badonicus, was won about 470.¹

These references, in my opinion, have little historical value. They are simply the background, artfully painted in by Geoffrey. He arrived at the date of 470 by taking the Nennian date of 428 for the Saxon Advent, and then accepting the Bedan misinterpretation of Gildas, by which Mons Badonicus took place some forty-odd years afterward. Since twenty-one years elapsed between Mons Badonicus and the last of Arthur's battles, Camlann, the proper Galfridian date for Camlann would be c. 491. But when Geoffrey does come to Camlann (bk. XI, c. 2) he departs from his usual practice and does not merely imply a date, but definitely states it—and the date is not 491 but 542.

This glaring inconsistency requires explanation. The simplest

¹ A. W. Wade-Evans, *Welsh Christian Origins*. Oxford, 1934, pp. 101-117; 'The Chronology of Arthur'. *Y Cymmrodor*, 1910, XXII, 125 ff.

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theory would be that after Geoffrey's 'History' was almost completed he came across a document giving the date 542 for Camlann. Such a bit of information was too good to leave unused, so he worked it in, either not noticing or not caring that it made nonsense of his carefully-planted references to Leo and Simplicius.

The date of 542 for Camlann implies a date of 521 for Badon (Mons Badonicus). Now this is also the date implied by the (eleventh century?) Franco-Breton 'Chronicle of St. Michael's Mount',² which notes the birth of St. Gildas under a year which corresponds to 521. Gildas himself tells us that he was born in the year of Badon (De Excidio, c. 26). So here we have two separate authorities, of the twelfth century or earlier, dating Badon in 521.

The agreement between Geoffrey and the Chronicle of St. Michael's Mount (Mont St. Michel in Normandy, not the one in Cornwall, or in Central Brittany) points to the former existence of a document, probably Breton in origin (Geoffrey claimed to derive his whole book from a Breton source) which dated Badon in 521, Camlann in 542. This is curiously close to the dates given by the *Annales Cambriae*,³ which are 517 and 538. The four-year discrepancy is a valuable clue. It suggests that the original Breton writer responsible for the dates of 521 and 542 relied on a 'computus' which included an item; '... From the Reign of Vortigern to the Battle of Badon 72 (or 71) years, ... from the Battle of Badon to the Battle of Camlann, 21 years ...'.

Our hypothetical Breton wished to find a point of contact between this and other annalistic material. He found it, in Bede or the Anglo-Saxon Chronicle, or some derivative, in the notice of Vortigern's part in the Saxon Advent there dated to c. 449-450. Adding 72 to 449, he arrived at his date for Badon.

² Edited by Labbe, *Nova Bibliotheca Manuscriptorum Librorum*. Paris, 1657, I, 349. The 'Chronicle' itself is Avranches Library MS. 213. It is reprinted by Migne Patrol. Lat., CCII, 1323.

The relevant passage runs:

CCCCXXI. Natus est S. Gildas. His diebus fuit Artus Rex Britannorum fortis, & facetus.

Of course CCCCXXI is 421, not 521. But the date intended is 521, as demonstrated by a later entry:

DXXXIV. Occisus est Cauallonus Rex fortissimus majoris Britanniae.

Ca(d)uallonus is unquestionably Cadwallon ab Cadfan of Gwynedd, who was defeated and killed by Oswald of Bernicia in 634.

³ I follow the interpretation given by H. M. and N. K. Chadwick, *The Growth of Literature*. Cambridge, 1932, vol. I, p. 147, note 1.

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But we know, what he did not, that the Saxon Advent took place in the *fourth* year of Vortigern's 'reign'.⁴ The true date implied by his Computus, then, was 517, the same as that given by the *Annales Cambriae*.

The second item concerns Cheldricus, who according to Geoffrey (bk. ix, chapters 1-5) was a Saxon chief fighting against Arthur, slain by 'Duke Cadwr of Cornwall' (Cadwy of Dumnonia, the Cato of the 'Vita Carantoci') in the retreat from Badon. Several writers⁵ have identified Cheldricus with King Cerdic of Wessex (more properly of the Gewissae).

I was formerly inclined to regard this equation as probably mistaken, and in any case unhistorical, since according to the Anglo-Saxon Chronicle Cerdic lived on until 534. But more recently, a study of early West-Saxon chronology has led me to the conclusion that whether or not he took any part in the Badonic campaign, Cerdic *did* die in 517 or 516.

The *Anglo-Saxon Chronicle* says that Cerdic landed at Cerdices Ora (Totton, on Southampton Water⁶) in 495. The Parker Preface, a much older document, gives a date which may be interpreted either as 494 or 495. But the A.S.C. says that it was not until 24 years later that Cerdic became king, while the Parker Preface gives an interval of only six years, which seems much more probable. Cerdic's reign would begin in 500 or 501, then, instead of 519. A reign of 16 years would terminate in 516 or 517. This would leave an awkward gap between Cerdic and Cyneric, whose reign began either in 534 or *c.* 546. The A.S.C. tells us that Cyneric was Cerdic's son, but the earliest form of the West Saxon royal genealogy⁷ makes Cyneric the son of Creoda, son of Cerdic. This Creoda may have reigned between Cerdic and Cyneric. If so, why does the A.S.C. omit him? Was it because Creoda's name was associated with unhappy memories? Bede says that the compilers of Northumbrian king-lists deliberately omitted the names of Osric of Deira and Eanfrith of Bernicia, the apostate successors of King Edwin, both of whom were killed by Cadwallon of Gwynedd in

⁴ Nennius, *c.* 66. It is worth noting that this is followed by an attempt to calculate the date of an apparently unspecified event, which seems to be 71 years later.

⁵ R. H. Fletcher, 'Arthurian Material in the Chronicles'. (Harvard University) *Studies and Notes in Philology and Literature*, x, 1906, p. 185.

F. J. Snell, *King Arthur's Country*. 1926, p. 106.

⁶ O. G. S. Crawford, 'Cerdic and the Cloven Way'. *ANTIQUITY*, 1931, v, 441 ff.

⁷ MSS. Tiberius B.I. & A. vi.

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634. Their brief reigns were a time of great humiliation and suffering for the Northumbrians. Cadwallon had established himself securely at York, and was mercilessly harrying his nominal subjects. If Oswald, younger brother of Eanfrith, had not surprised him by a night attack, the Welsh king might have made good his boast, that he would make Northumbria a Cymric land. Did Creoda, coming to the throne of the Gewissae following the crushing Saxon defeat at Mons Badonicus, sue for peace, and reign as the tributary of some British king, under what the later West Saxons regarded as humiliating conditions, whose memory they did not care to perpetuate?

Neither the A.S.C. nor the Parker Preface says anything concerning the circumstances of Cerdic's death or the place of his burial. However, a charter dating from the time of King Edward the Elder (900-924) mentions a Ceardicesbeorg, 'Cerdic's Barrow' at Stoke near Hurstborne in northwest Hampshire, between Whitchurch and Andover.⁸

If this place-name indicates the actual 'barrow' of Cerdic, we have the additional fact that he was buried close to the northwest frontier of his realm. Now if Mons Badonicus was somewhere in the direction of the Cotswolds, the Whitchurch-Andover region is just where we should expect a Gewissan contingent, escaped from the slaughter, to re-enter their own territory. If they were carrying back with them the body of their king, mortally wounded in a rear-guard skirmish, it is just about here that we should expect them to pause and bury their slain leader, perhaps as a 'secondary interment' in a barrow already centuries old. Or, if the victors had found Cerdic among the slain, and, chivalrously returned the body of their great foe to his own folk, it would be somewhere in the same region that the parties would meet.

If we accept the evidence of the earliest West Saxon documents, there is no chronological difficulty whatever in supposing that Cerdic fought at Badon and was killed as he fled homeward, and the place of his burial suggests that he was on or beyond the Brito-Saxon frontier at the time of his death.

⁸ J. Kemble, *Codex Diplomaticus Aevi Saxonici*, No. 1077. [Also Birch, *Cart. Sax.* II, 594. For an attempted identification of these bounds see my *Andover District* (Oxford, 1922), pp. 69-72. Unfortunately the early part of these bounds is very difficult; but it seems certain that Ceardices beorg lay somewhere not far west of the junction between the Hurstbourne and the Test, perhaps in the neighbourhood of Faulkner's Down. Major Allen has searched this locality from the air without finding any signs of unknown barrows. O.G.S.C.]

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Chronology and geography offer no obstacle, but what of philology? Cheldric and Cerdic have the letters C, E, D, R, I and C in common, and all but R in the same order, yet they are by no means the same name. Cerdic is the A.S. rendering of British Coroticos, modern Welsh Ceredig. Cheldric appears to be purely Teutonic=Childe-Riks, Frankish Childeric(h), A.S. Hildric, 'War-King'.

Now it is not at all likely that Geoffrey himself would have altered Cerdic to Cheldric. He had read Henry of Huntingdon and William of Malmesbury, both of whom give an account of Cerdic taken from the A.S.C. But a Franco-Breton scribe might well be totally ignorant of Cerdic the Gewissan, but well acquainted with the name of the Frankish Childric, father of Chlodovech. He might think that in changing Cherdic (as a Breton would probably have written it) to Cheldric he was merely rectifying the blunder of some careless copyist. The form Cheldric is a strong indication that for his account of the Badon campaign Geoffrey relied upon a document of Continental origin. If he had drawn his material from Welsh tradition, written or oral, we should find some reference to 'Osla Gyllellfawr (Great Dagger) the man who fought against Arthur in the Battle of Baddon'.⁹ But Osla, who must have figured largely in the early Welsh Arthur-saga (he appears in the prose tales of Kulhwch and Rhonabwy as well as the genealogical documents cited by Anscombe) is an even more mysterious figure than Cerdic. He has been tentatively identified with Cerdic's reputed grandfather Esla, with King Oisc of Kent, and with Ossa, the grandfather of King Ida of Bernicia. The last theory is perhaps the best, as an Eda appears among his reputed descendants. Since Ida seems to have started from the Fenland region, his followers including Lindisfaras (Lindisfarne) from Lindsey and Gyrwas (Jarrow) from the Fens, it is not quite impossible that his grandfather took part in the campaign of Badon, as the leader of a great host drawn from many regions, with Cerdic of the Gewissae for one of his most powerful allies. If so, the host that Arthur shattered at Badon Hill must have included contingents from almost every Teutonic settlement between the Wash and Southampton Water.

P. K. JOHNSTONE.

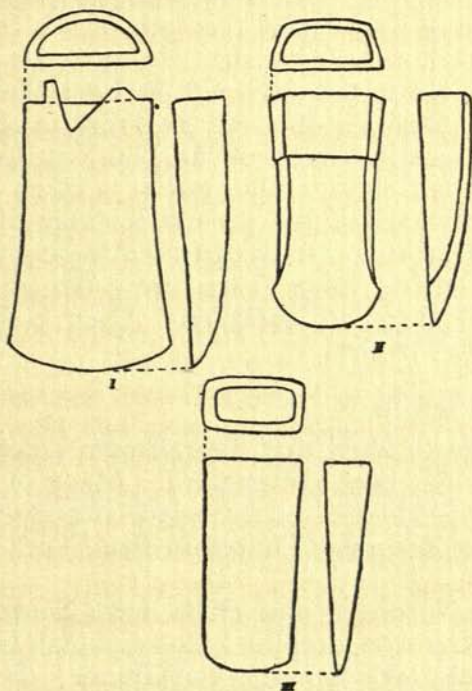
SOCKETED CELTS IN CHINA

Some years ago Prof. C. G. Seligman pointed out the similarity between the socketed celts of the European Bronze Age and those of

⁹ Alfred Anscombe, 'Indexes to Old-Welsh Genealogies'. *Zeitschrift für Celtische Philologie*, II, 1904.

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Ancient China in his article 'Bird-chariots and Socketed Celts in Europe and China' (*Journ. Roy. Anthr. Inst.*, 1920, vol. L). He later suggested a more definite dating in 'The Roman Orient and the Far East' (*ANTIQUITY*, 1937, XI, 5). He says there, 'the socketed celt probably reached China some five or six hundred years B.C.' (p. 7). In a note published in *ANTIQUITY*, 1938, XII, Prof. Seligman admitted that the actual date was several hundred years earlier, *i.e.* in the 12th or



SOCKETED CELTS FROM THE RUIN OF THE CAPITAL OF THE SHANG-YIN DYNASTY AT AN-YANG
After Dr Li Chi ($\frac{1}{2}$)

11th century B.C. 'In other words there is now evidence that the socketed celt existed in China towards the end of the Shang-Yin dynasty.' (p. 86). But he based his new conclusion mainly upon a raised triple sign on a Chinese socketed celt in his own collection, and seems still unaware that conclusive evidence was discovered, as a result of scientific excavations, in 1929, and published in 1930 (unfortunately only in Chinese). This note is written in order to draw attention to this archaeological evidence.

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In the *Preliminary Report of Excavations at An-yang*, part 2 (1930), Dr Li Chi, the field director of the excavation, published a bronze socketed celt discovered in the ruins of the capital of the Shang-Yin dynasty, which was deserted in 1122 B.C. according to the historical tradition. He noted Prof. Seligman's article in JRAI., 1920. But the European type occurred only during the Late Bronze Age, which began about 1300-1250 B.C. (V. G. Childe, *The Bronze Age*, p. 59). He rightly questioned the validity of a hypothesis which implies an extraordinary speed of diffusion of this type of celt from Europe to the Far East in prehistoric times (p. 246). In 1933, Dr Li Chi published three socketed celts from the same site in his article 'Five Kinds of Bronze Implements from Yin-hsu and the Problem of their Analogues' (in the volume of essays in honour of Mr Tsai Yuan-pei on his attaining the age of 65). The following illustration is taken from figure XI of his article. He summarized the general features of the bronze celts from the ruins of the Shang-Yin dynasty at An-yang as follows: 'Most of them are asymmetrical when viewed from the side (*i.e.* adze rather than axes). All of them are socketed. Their edge is convex, and slightly splayed out'. (p. 97).

N. SHIAH.

GUERNSEY MUSEUM

It is good news to learn that the museum accommodation in the island of Guernsey has been reorganized. Many of us remember the old museum and its valuable but crowded collections. The new one was opened in a deconsecrated church in Peter Port on the 11th June, 1937.

Previously the furniture and china were housed in the Priaulx Library in Candie Gardens, and the Lukis material, both archaeological and geological, in the Lukis House in the Grange. It was impossible to exhibit the geology, which is now on view for the first time and well arranged and labelled.

It was considered high time a Museum was founded to house all this material. Little money was available but extra cases have been bought at local auctions. Where necessary automatic release electric lights have been fitted. Great efforts are now being made to improve the exhibition of Lukis material and it is hoped that one day the manuscripts will be arranged in card-index form.

Any Guernsey people outside the islands who have knowledge or specimens are asked to communicate with the Curator. Reprints of C.I. subjects are also gratefully received.

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One of the advantages of the reorganization is that it is now possible for any student on application to have access to the Lukis manuscripts. Those who have seen them know what a valuable collection of material is available there. The Lukises were 19th century workers who occupied much of their time with making plans of megalithic monuments, and although of course a great many of them still survive, the plans are of great value in other cases where they are the only record of a vanished monument.

ARCHAEOLOGY IN THE UKRAINE, U.S.S.R.*

The following notes on eight expeditions were received at Field Museum of Natural History in a private communication to Henry Field from N. I. Īachmenev, director of the Institute for the History of Material Culture (IMK), of the Ukrainian Academy of Sciences (ANU), Kiev. The translation was made by Eugene Prostov.

1. In connexion with building activities in Kiev excavations were conducted at several points, under the leadership of N. I. Īachmenev. A site selected for the Art Academy building, formerly occupied by the Desiatinnaia church, was discovered to be the feudal centre of the princes of Kiev, and the excavations disclosed the remains of many historical structures: churches, palaces, fortifications, dugouts, and various industrial buildings. Many implements, vessels, ornaments, and coins were found. To the north two large seventeenth century brick kilns were uncovered. They contained thin bricks typical of the eleventh to the thirteenth century, as well as large bricks characteristic of the seventeenth century. It is thought that the thin bricks were used for repairing early feudal monuments.

The ancient moat surrounding the town on the east and south was investigated, and a Varangian burial, containing a male and a female skeleton of the ninth or tenth century, was found in a deep, rectangular grave faced with logs.

Earth-dwellings containing early feudal pottery of the eleventh to the thirteenth century were also discovered in the neighbourhood, as well as grain storage pits containing datable materials.

* For additional notes see Henry Field and Eugene Prostov: 'Recent Archaeological Investigations in the Soviet Union', *American Anthropologist*, xxxviii, 274-78, April-June 1936; 'Archaeology in the Soviet Union', *ib. id.* xxxix, 470, July-September 1937; and 'Archaeology in the U.S.S.R.', *ib. id.* xl, 665-6, 1938.

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In the southeastern section of the ancient acropolis of Kiev, where according to historical evidence the palace of Prince Vladimir (tenth or eleventh century) was located; the excavations disclosed burnt remains of stone walls and frescoes and mosaics. The associated materials date the destruction of this building in the thirteenth century, during the Tartar invasion. Human skeletons were piled one upon the other.

2. The Vyshgorod Archaeological Expedition (F. N. Molchanovskii, leader) disclosed interesting materials for the history of early feudal architecture, metallurgy, a ceramic technique, and art. The walls of a church known from the chronicles of the eleventh century were discovered; they revealed traces of the fire of 1240, which destroyed them. This date also marks the year in which Kiev was destroyed.

3. In the Chernigov region Paleolithic sites were explored near Chulatovo and Novgorod Seversk, and the shores of the Desna River were studied for other stations. At Novgorod Seversk, T. G. Pidoplichka found gigantic flint implements in a Paleolithic deposit. The largest was 45 centimetres long and weighed 8 kilograms. Another was 44 centimetres long and weighed 8.5 kilograms.

These sites shed light on the method used in dressing carcasses of the rhinoceros, the mammoth, and other extinct animals. An incised rib (sixty centimetres in length) of *Elephas primigenius* was found. M. V. Voevodskii continued the excavations at Chulatovo, where the fronto-occipital part of a Paleolithic skull came to light. A skull showing signs of having been used as a vessel was found at the same site. [Human calvaria which had been used as ceremonial or drinking bowls were discovered in the Magdalenian deposits at Le Placard, Charente, France. (Cf. also modern Tibet)].

4. The Azov (Black Sea) region was studied by an expedition under the leadership of O. N. Bader, of Mogaimk, Moscow Anthropological Museum.

5. The Tripolje expedition (S. S. Magura, leader) continued its 1934-5 excavations at Khalep'e, near Kiev, and at Vladimirovka, near Uman. Well-preserved clay dwellings of several types were disclosed near Khalep'e. Larger structures were of complicated design and consisted of several rooms. The construction of *gorodishches* was also studied. The inventory included implements of stone, horn, and bone and more than thirty anthropomorphic figurines, including several specimens possessing hermaphrodite characters. The bones of domestic and wild animals were also unearthed.

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A house model (287 by 255 by 21 cm.) raised on high legs was found in a building near Vladimirovka. Opposite the door in the front wall was a round window. The floor and both sides of the walls were decorated with bands. Three baked clay platforms, 20 centimetres high and 1.5 metres in diameter, were found on the floor of the same building. Each platform had four large semicircular projections, oriented to the four points of the compass. The prominences were decorated with concentric circles. This structure was probably of ritual significance.

6. An archaeological expedition excavated seven Scythian tumuli near Nikopol (Nicopolis). According to L. D. Dmitrov, of IIMK, and Brîusov, of the Moscow Historical Museum, valuable materials illustrating the differences between the Scythians and the Greek colonists were unearthed.

7. The Olvia (Olbia) Expedition, sponsored by ANU and GAIMK (L. M. Slavin, leader), continued excavations at this important Black Sea site.

8. Another IIMK expedition (V. P. Telichko, leader) studied sites of early metallurgical industries in the Polisie area of western Ukrainia. The remains of eighteen mines and four factories of the eighteenth century were disclosed. Many metal implements, including hammers, anvils, and cauldrons, were found. These materials will make possible the study of metal working technique of the Feudal Era.

Telichko also excavated three *gorodishches* near Gorodskoe in the Zhitomir region. Charred remains of implements, weapons, and dwellings were mingled with human skeletons. From this evidence and from references in historical chronicles, these mounds were thought to be the remains of settlements destroyed in 1257 by the punitive expedition of Daniel [Danilo], Prince of Galich.

HENRY FIELD AND EUGENE PROSTOV.

A LATE-RHAETIC HILL-TOP CAMP IN THE TYROL

In 1937 I discovered a camp in the little mountain-village of Birgitz near Innsbruck, at a height of about 850 m. The outlines of buildings could be plainly recognized in its interior, so I decided to examine it more closely, and the first excavations were made in September 1938.

The camp lies on a hill of moraine, which falls away fairly steeply in all directions, but most of all to the north. A dry wall surrounds

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the top, but on the north this seems to have served not so much as a fortification as a protection against falling over the precipice. The maximum area enclosed is 160 by 55 metres. At the foot of the hill also, on the south side, remains of a wall can be seen, but these have not yet been investigated.

On the top of the hill, where it inclines southward, thirteen platforms of varying sizes lie within the circumvallation: these were cut out of the ground to provide level spaces for housing sites. We examined the largest of these, and struck a dry wall 23 m. long, very neatly fitted on the inner side, while on the outside it was sunk in the earth. In the east wing of the house this wall receded for nearly 3 m. to a depth of 2 m. Here it was neatly built on the outside also, in contrast to the rest of the wall, and obviously represented a ceremonial entrance. In the west wing there was a simpler entrance. The wall was about 1 m. high, and was formed of three or four layers of stone, but with the upper part of wood. In the interior, which had six rooms and a passage, the lowest beams of the party-walls were still in position, generally supported on a row of stones. An earlier stage in its construction was recognizable, and showed the outer frontal of the house as two metres less in breadth (about 6 m. altogether, as opposed to the 8 m. of the later building) and was constructed only of wooden beams resting on a base of stone. The side-walls of the house were of similar construction to that of the party-walls, for a number of flags could be seen in the floor, with remains of beams upon them. The method used in building the back was not very clear; no base of stonework was apparent, nor any definite traces of fitted beams. The building may have been open at the back, and only furnished with posts to support the roof. Behind it there was a further space about 1.5 m. broad, and finally a sustaining wall against the moraine. The roof was probably of the lean-to type, sloping towards the outer wall.

The finds include a number of clay sherds, an iron spit, a piece of bar-iron, an iron fibula, a razor, fragments of glass bracelets, whet-stones, and a quern. All are of the same period, the last century B.C. They do not belong to the La Tène culture, but rather to the local Illyrian-Rhaetic development of the Hallstatt culture in the Inn valley.

Complete excavation is necessary for a definite judgment of the settlement, and this will occupy several years. It is clear, from the intensive traces of burning in the house which has been excavated, that it was destroyed by an enemy attack, probably in 15 B.C. when the Romans forced their way into this region.

OSWALD MENGHIN.

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HAITHABU AND THE DANEWERK

The Viking settlement of Haithabu has a special place among the prehistoric monuments of northern Europe. It lies on the Schlei, south of the city of Schleswig; the earth wall surrounding it is still in existence, and to the west is the Danewerk, a dyke, or linear earth-work, 15 km. in length. It is important because of its historical associations, quite apart from its excellent state of preservation. The Frankish chronicles first mention it, under the name of Sliesthorp, in 804, and in 808 we are told that king Göttrik had a great wall constructed there.

The origin of the settlement can be dated from the Viking expeditions which, besides covering western Europe, reached the east and the German coast as well. These resulted in the foundation of a Norse empire, with the whole of north and northwest Europe forming a single great trading-unit long before the existence of the German Hanse-towns; before the migration of peoples, the Baltic basin had been both culturally and economically separate from that of the North Sea. The problem of connecting the two seas now assumed importance; the natural route led from the Baltic past the Danish islands, round Cape Skagen to the North Sea, but this was not regularly used until the Hanseatic period, and in Viking days trade took a different course. The peninsula of Jutland, forming a great land-barrier between the two seas, is very narrow at several points, and particularly so at Schleswig between the estuary of the Schlei and the line of the rivers Treene and Eider, where the distance across is only some 16-17 km. from water to water. This land-bridge was crossed in the Viking period; a canal was unnecessary, so long as portage was possible. There was in fact at this spot a sort of precursor of the Kiel canal, and it is exactly at the beginning of the land-route to the west that Haithabu is situated. The Schlei region was further important as constituting the frontier between the Angles to the north and the Saxons to the south; at the time when Haithabu was founded, the Danes established themselves in the old Anglian settlements. The boundary between the two peoples was determined by natural conditions; on the west the peninsula is deeply cut by the Treene and Eider, and on the east by the indentations of the Schlei and the Eckernförde Bight. An ancient road was already in existence here in the Stone Age, running from north to south, the great army road linking North Germany with Jutland. At Haithabu this crossed the trade-route from west to east, adding to its importance as a frontier-town.

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The town was surrounded by a great semicircular wall, originally constructed of earth, with a perpendicular wooden wall on the further side. Its present height of 11 metres took some years to attain, for it can be shown to have been raised on nine occasions. There were two main gates, north and south, which may have been connected by a road. A brook ran through the town from west to east. To the north of the semicircular fortification there is a smaller fort, on an eminence, surrounded by a flat wall, containing numerous tumuli in its interior.

Since 1900 the site has yielded rich material. Two runic stones, both giving the name of the town, show that it is actually the ancient Hedeby, well-known in nordic saga. The stones were discovered in front of the town, and were a monument to warriors who fell there. It is clear from the finds that there was a large settlement here, with wide commercial interests, for English and German coins were found, as well as Arabian silver coins, and ornaments from the Baltic. Objects with animal-ornamentation show the great influx of Scandinavian peoples. The west is particularly well represented; the Rhineland did much northward trade in objects such as clay vessels, glass-ware, trinkets, and mill-stones. A small gold disk, decorated with a cross and interlaced bands, seems to have come from England. The finds date from about 800-1050.

The place was also known as Sliesthorp and Sliaswic, as well as Haithabu. We learn from an Anglo-Saxon chronicler, Ethelwerd, writing in the tenth century, that the capital of the old province of the Angles, lying between the country of the Saxons and of the Jutes, was called Schleswig by the Saxons, Haithabu by the Danes. The town had a changeful history. The Danish king Göttrik made it into a great commercial centre about 800; between 890 and 900 a Swedish royal line was established there; in 934 it became part of the German Empire and the seat of a Saxon count; in 983 or 1025 it was ceded to Denmark, and about 1059 it was destroyed in a great fire. After this the inhabitants built a new town, Schleswig, on the north bank of the Schlei. The Danewerk, adjoining the town on the west, forms a frontier-barrier between north and south. Several earthworks still remain today, but their age cannot be definitely established in detail. The position was cleverly chosen, in view of the narrowness of the isthmus at this point. The original dyke, probably that now called the *Kograde*, became necessary in 808 when Charles the Great overran the region north of the Elbe in his attempt to destroy the remnant of the Saxon people. Göttrik built the dyke in case of further attacks directed northwards.

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Its course is easily recognized, and in some places can be supplemented by air-photography. The individual history of different parts of the dyke is not yet clear, but the main part of the Danewerk shows nine different periods of construction. From a point at the west end of this fortification, near the modern village of Hollingstedt, it was possible to pass by boat via the Treene into the Eider and so to reach the North Sea: perhaps in Hollingstedt we may recognize the ancient port from which the Angles sailed to England.

The date of the destruction of Haithabu is uncertain. It may have been destroyed by Harald Hardrada in 1050, perhaps not until an attack by the Wends in 1066. Its importance lay in its key position on the trade-route from east to west, and after its destruction its place was taken first by Schleswig and then, 50 years later, by Lübeck. The significance of Haithabu for Danish history is that it formed the central point of the first great Danish empire on the mainland, and for Germany it represents the first Baltic harbour of the German Reich.

DR HERBERT JANKUHN

WALLOP

In an interesting note in *ANTIQUITY*, December 1938, Mr Philip P. Graves discusses the meaning of the river name Wallop, and supports the identification of this place with the *guoloppvm* or *guoloph* of the *Annales Cambriae* (*Cymmrodor*, ix, 1. p. 152), a well-known comparison which, as Mr Graves remarks, has been unduly ignored by Ekwall. Whatever the real etymology of Wallop may be, however, the connexion proposed on the ground that the Wallop is a winter bourne cannot be right. The argument, which is based on a note by Rhys in *Cymmrodor*, xviii, p. 73, is that *guoloppvm* or *guoloph* means 'empty', and the Wallop is liable to dry up. But there are insuperable objections to this. Rhys did not intend to say that there was a word pronounced *guoloppvm* or *guoloph* and meaning 'empty', but that these were mere spellings of a hypothetical **guolom*, in which the *-m* was a nasal *v*; and this he equated with the Irish *folamh*, 'empty'. If so, when the Anglo-Saxons came to borrow the name for the river they would have called it something like Wallom,¹ certainly not Wallop, as the word

¹ For the borrowing of early Welsh nasal *v*, spelt *m*, as pure *m* in Anglo-Saxon, cf. *Caedmon* (7th cent.) and *Elmet* (8th cent.), both from Welsh names with what was probably already pronounced nasal *v* by this time (older *m*). The nearest the English could get to this peculiar sound was evidently a simple *m*.

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suggested by Rhys had no *p*. He explained the *ph* of the MS by supposing that the scribe used it 'inexactly' to stand for the nasal *v*. This would be a freak; the nasal *v* is always spelt *m* in Old Welsh, not *ph*, and the sound was quite different. As for the *pp*, Rhys took it that this was a more antique spelling for *ph*, as it might well be. It could never stand for Old Welsh *m*. In any case, neither *guoloppvm* nor *guoloph* can have anything to do with **guolom*, if such a word ever existed; nor could **guolom* be borrowed as Wallop in English. The possibility still remains that *guoloppvm* and *guoloph* do refer to the river Wallop; but not for the reason suggested. KENNETH JACKSON.

PLACE-NAME SOCIETY

Through an unfortunate slip of the pen the annual subscription to the PLACE-NAME SOCIETY was stated in our December number to be 25 shillings, instead of 15 shillings. This considerable difference will lead, we hope, many to become subscribers who may have been deterred by the mention of the wrong amount.

Reviews

MARBURGER STUDIEN, herausgegeben von ERNST SPROCKHOFF. *pp.* VIII, 267 and 122 plates. Darmstadt: L. C. Wittich Verlag, 1938.

Ten years ago Gero Merhart von Bernegg was appointed the first 'ordinary' professor of Prehistory in Germany. By then he was already famous for his writings on Siberian antiquities and his subsequent work at Innsbruck. He did not use the opportunity, created by academic advancement, for increasing his fame by a stream of learned articles and books. Instead he seemed to vanish from the clamorous world of regular authors; for he set himself to create a sober school of scientifically trained archaeologists. The high quality of the thirty-one essays, composed by Merhart's students to celebrate the tenth anniversary of his professorship, provides eloquent testimony to his success and concrete proof that his labours are recognized—proof too that the sound old traditions of German scholarship still survive despite the strains of the last five years. The collection thus inspired is indispensable to any archaeological library. Its scope extends from Western Asia to Ireland, from the New Stone Age to the Roman period. Only a few essays can be singled out for mention, and the selection must inevitably depend rather on the momentary interests of the reviewer than on the merit of the contributions themselves.

Two essays deal with the general question of prehistoric chronology as based on Oriental parallels to central European Early Bronze Age (Aunjetitz) types. K. Bittel (pp. 9–19) illustrates with first class photographs all the bossed bone plaques found at Troy and shows that in the light of recent excavations none can confidently be assigned to Troy II. Thus one step in Hubert Schmidt's notorious argument for an inflated chronology—the virtually identical plaques from Siculan I tombs and from Hal Tarxien in Malta—'goes west'. Bittel then proceeds to discuss Oriental parallels to Aunjetitz types. The datable knot-headed pins from Cyprus and Syria belong to the period 1800–1600 B.C. (an example from Kusura C would fit in here too, but the reliably dated predynastic example from Badari in Egypt is not mentioned), and the ingot-torques also fall between 2000 and 1700. So 1800–1700 B.C. would seem a maximal date for the beginning of the Danubian Early Bronze Age. Uenze (pp. 243–8) also reaches a date about 1700 B.C. for the beginning of the period which he regards as older than the Shaft Graves of Mycenae—in opposition to Åberg and Reinecke, denying incidentally that the 'halberd' from Shaft Grave VI is really a halberd.

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Buttler's fully documented account of 'Neolithic trade' principally along the 'Danube Thoroughfare' provides the theoretical justification for the argumentation used by Bittel. Though 'trade' dealt mainly with 'luxuries' and not with necessities, even pots were transported for long distances—Tisza vases of the type manufactured round Szeged to Silesia and across the Danube to Vinča. *Spondylus* shells and ivory from the south found their way even to the Middle Rhine. Ring-beads of faience (mistaken by the excavator for copper) are reported from all levels at Vinča, and Buttler mentions also cruciform and 'hour-glass' beads of faience from Kis-Zombor in the Banat. But cruciform beads are regularly found in Bronze Age graves, and the hour-glass one is presumably a fragment of a segmented bead such as are common in Early Bronze Age graves round Szeged and are now known to be identical with our Wiltshire specimens. Kis-Zombor, a stray find in the Museum at Timisoara, is probably wrongly dated to the Stone Age despite the parallels from late predynastic Egypt cited by the author.

To the elucidation of the 'megalithic problem' Leisner's dissertation, based largely on the study of unpublished material in the late L. Siret's collection and notes, is an important contribution. An unsuspectedly large number of corbelled tombs in Almeria and of megalithic cists with short entrance-passages in Granada turn out to have been provided with port-hole entries, and two unrecognized examples from Portugal are described for the first time. It is surprising that most writers on megalithic architecture have ignored southeast Spain, which really rivals Sardinia as a centre of port-hole slabs. Leisner notes how the port-hole slab gives the same effect as the rock-cut entries to the artificial grottos of Palmella, as Kendrick did in comparing the Paris cists and the Marne grottos. His photographs admirably illustrate Hemp's thesis on the relation of 'caves' to megaliths, but he rigidly abstains from comparisons outside the Peninsula and from speculative interpretations.

Two papers referring to the British Isles are among the weakest in the volume. F. C. Bursch devotes pp. 20-25 to 'The Question of the Deverel Urns in Holland'. He uses 'Deverel urns' in Doppelfeld's sense of globular, barrel and bucket urns for which Lower Rhenish prototypes may plausibly be claimed. But his text and map fail to distinguish the several components recognized by Doppelfeld and Hawkes, his figured examples are not identical with English types, and he makes no reference to any British author since Abercromby. His conclusion is that the 'Deverel urns' of Holland represent the vases made by the native population in imitation of models introduced by Urnfield peoples, at the transition from Bronze Age to Hallstatt times; one part of this native population was absorbed to produce the lower Rhenish chip-carved pottery, another crossed over to England. J. Raftery outlines a division

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into four phases of the Irish 'Iron Age' from 500 B.C. to A.D. 450. Little documentation is offered, and the Ballintoy caves are cited as a 'Hallstatt settlement' though pottery like that from the caves has been found with a bracelet of the Lagore type, attributed to the Dark Ages.

Apart from Nestor's article on decorated bronze battle-axes from West Rumania which convincingly defines the chronological position of these handsome weapons, the remaining contributions start from German finds. But of course they often give rise to conclusions of much wider import as when Holste insists (p. 101) that the Middle Bronze Age tumulus culture is no direct continuation of the local Early Bronze Age tradition; he looks for the source of the new impulse to Hungary as did the present reviewer ten years ago.

V. GORDON CHILDE.

KELTEN UND GERMANEN IN BÖHMEN. By LEONHARD FRANZ. *Brünn, Prag, Leipzig, Wien: Verlag Rudolf M. Rohrer (in series 'Das Sudetendeutschtum'), 1938. pp. 37, 2 plates, 1 map. 3.35 marks.*

This is a 28-page monograph, with 9 pages of notes appended, on the Celtic and Germanic occupations of the pre-Slav Bohemia. It forms part of a series entitled 'Das Sudetendeutschtum: sein Wesen und Werden im Wandel der Jahrhunderte' and appeared in the earlier part of 1938. The other volumes in the series are devoted to various aspects of the cultural and political history of the Sudeten Germans in historic times, and Dr Franz opens with a discussion of the value of archaeological material as a key to the prehistory of tribal cultures, in this case of the Germanic and Celtic groups recognized by Roman sources in Bohemia in the first centuries before and after Christ. In any analysis of Celtic origins an important part must be assigned to the Tumulus culture, which in Central Europe answers to our own Round Barrow cultures of the Early and Middle Bronze Age, and he rightly relates how in west-central and southwest Bohemia, as elsewhere, this ran continuously parallel first to the Aunjetitz and then to the Lausitz and Urnfield civilizations, to issue directly in the Middle and Late Hallstatt Tumulus cultures which in their turn develop into the undisputed 'Keltentum' of Early La Tène. The question of continuity between Early and Middle La Tène indeed is not broached; but no one doubts anyhow that the latter also was Celtic, and so we follow on straight to the powerful and well-defined Late La Tène culture of this region, with its great hill-towns of which the most famous is the Hradischt of Stradonitz, above the Beroun not far from Prague. This complex industrial civilization, as is well known, is shown by its rich and varied archaeological material to be almost identically similar to that of the Mont-Beuvray, the Bibracte of Julius Caesar's Gaulish allies the Aedui, and Franz is inclined to explain it by a

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strong reinforcement of the native Bohemian Celts by immigrants from Gaul. He suggests that these may have been refugees from Caesar, and it is quite true (though he clearly does not know all the evidence) that most of the material in question, especially the distinctive painted pottery, is post-Caesarian not in Bohemia merely, but in both France and Switzerland. Thus the Bohemian Late La Tène civilization belongs in the main to the latter half of the 1st century B.C., but against the archaeological contrast which it makes with its Middle La Tène predecessor has to be set the fact that the same Celtic tribe-name persists through both, that of the Boii. This must stand for a political and ethnic continuity, and Déchelette was surely right in insisting, as he did against Pič, that the links between Bohemia and Gaul were commercial and industrial rather than the work of a mass migration.

North Bohemia, however, had already been penetrated by Germans coming through the Elbe defile from Saxony, and the author lays great stress on the case to be made for this from the cremation-graves of the Bodenbach district—which has actually been recognized by Czech as well as by German and foreign writers for a considerable time. (He is nevertheless careful to dissociate himself from the claim that the whole of Bohemia had been occupied by the Germans as early as 300 B.C., which he quotes from a distribution-map shown at the 1937 'Lebendige Vorzeit' exhibition in Berlin). This Elbe-Germanic culture he would assign to the Hermunduri, the ancestors of the Thuringians, but the case for a continuous tribal history here down to the 6th century A.D. is somewhat frail, and is not reliably strengthened by Tacitus' statement (*Germ.* 41, 8) that 'the Elbe rises among the Hermunduri', for, as appears from the accounts we have of Domitian's Elbe expedition of between 7 and 2 B.C., the Romans tended to confuse the Elbe with its tributary the Saale, which of course does not touch Bohemia at all. And if there were Hermunduri in north Bohemia in the 1st century B.C., their migration and settlement on that same occasion on the upper Main may have made a break in the occupation along the Elbe; in fact Much suggested assigning this to the Marsigni instead. However, the main Germanic people to occupy Bohemia were in any case not the Hermunduri but the Marcomanni, whose migration here from further west under Maroboduus soon after 9 B.C. is one of the firmer date-points in the German history of the period. That they entered a land kept for the previous half-century as a tenantless frontier-march, as for example Much maintained, is of course mere artificial theory (based on tendentious combination of *B.G.* I, 5 and IV, 3); Franz rightly relies on Tacitus' statement (*Germ.*, 42, 2-3) that they won their new home by defeating the Celtic Boii—that is, the Late La Tène people of Stradonitz and the other *oppida* of its kind, who occupied as we have seen the whole southwest and centre of the country. The Marcomannic culture has received a good deal of attention,

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especially since Almgren's paper of 1913 (*Mannus*, v, 265), and there is not much here to add. It is clear that subjected Boii contributed a good deal to it, and their name survived compounded in that of the later Baiowarians of Bavaria, who sprang from these same Marcomannic conquerors of Boiohaemum. On the reasons for the success of the Dark-Age Slavs in making this Germanic Bohemia their own, Dr Franz has little to say, but hints that the German inhabitants may have been decimated by plague. His theme in general would really be more interesting on a larger scale; the trouble here is that the problems of 'tribe-archaeology' require such careful handling for their proper presentation that a summary in under 30 poorly-illustrated pages is almost bound to give an impression of unevenness and haste, however conscientiously the author has set about his task.

CHRISTOPHER HAWKES.

WESTGERMANISCHE BODENFUNDEN des ersten bis dritten Jahrhunderts nach Christus aus Mittel- und Westdeutschland. By RAFAEL VON USLAR. *Germanische Denkmäler der Frühzeit, herausgegeben von der Röm.-Germanischen Kommission des Deutschen Archäologischen Instituts*. Berlin: W. de Gruyter, 1938. pp. xvi, 272; 58 plates, 7 text figs. RM. 29.50 bound, 27.50 unbound.

FRÜHGESCHICHTLICHE FUNDE AUS WESTFALEN im Städtischen Kunst- und Gewerbemuseum Dortmund. By CHRISTOPH ALBRECHT. *Veröffentlichungen aus dem Städt. Kunst- und Gewerbemuseum Dortmund, Band I*. Dortmund: Fr. Wilh. Ruhfus, 1936. pp. 63; 14 plates, 39 text figs. RM. 10 (outside Germany, 7.50).

DIE ERDENBURG BEI BENSBERG, BEZ. KÖLN, eine Germanische Festung der Spätlatènezeit (Vorläufiger Bericht). By WERNER BUTTLER. *Germania*, Jahrg. 20, Heft 3 (July 1936), pp. 173-184.

The Germanic civilization of the prehistoric Iron Age culminated in the equivalent of the Late La Tène culture of Celtic Europe, in which it attained its maximum of settled territorial expansion in the Rhineland as the legacy of Ariovistus in the middle of the 1st century B.C. It there became increasingly assimilated to the Celtic culture it thus encountered at such close quarters, and the evidence to this effect of brooches and such small gear is borne out by that of the hill-fort building which the Germans adopted in emulation of Celtic practice. Dr Werner Buttler has recently begun excavation in one of the Germanic hill-forts of this period, the Erdenburg near Bensberg, some 15 km. beyond the Rhine nearly opposite Cologne, and his interim report for 1935 is the third of the works here reviewed. The oval hill-top is enclosed over an area of 230 by 165 metres by triple defences, steep-sided ditches fronting earthen ramparts of which the two outer each bore a single palisade, while the inner was

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surmounted by a stout double wall of timber. Special attention was devoted to the complicated entrance works on the west, skilfully excavated and convincingly reconstructed with an outer bridge over the approach-road and a strong tower flanking the inner gateway. The pottery and small finds point to a date in the latter part of the 1st century B.C., when the tribe responsible here must have been the Sugambri; and if, as has previously been argued for the Chatti of the Taunus region, it was with hill-forts like this that they opposed the invasion of the Roman armies under Drusus and Tiberius, the somewhat obscure course of those campaigns will have to be studied in a fresh light. With them Germanic 'protohistory' enters on a phase of transition, marked further south by the Marcomannic migration mentioned in the previous review, until finally after the Roman defeat under Varus and its sequels, there begins towards the middle of the 1st century A.D. the period of comparative equilibrium between Germany and the Empire which lasted until the final collapse of the Roman *Limes* about A.D. 260. To this period the two major works here noticed are devoted.

In the second, Dr Christoph Albrecht follows in the footsteps of Dr Stieren at Münster by assembling the contribution of his museum at Dortmund to the Germanic archaeology of Westphalia. He has already since embarked, in the next volume of the same series, on the long-awaited publication of the pre-war excavations in the great camp of the early Roman invaders at Oberaden, and there, and at the more famous camp of Haltern, Germanic material receives its first fixed dating points in Roman terms, within the Augustan period. Thereafter the process of thus establishing its chronology becomes part and parcel of the study of the native culture as a whole, and both the dating and the general character of this have been helped towards clearer emergence by what is here published, the cemeteries of Rünthe and Veltheim proving especially valuable. About 200 the current began to set towards the dynamic changes of the later 3rd and 4th centuries, and this region's population eventually becomes assignable to a place among the Franks; but it appears that the earlier known tribal divisions are hardly yet distinguishable on a basis of material culture.

It is through careful regional studies like this that the way of achievement in that direction lies; but in the first work on our title-list Dr von Uslar has carried the general advance over a much wider field, by bringing together the whole mass of the period's archaeological material from the entire territory between the Rhine and the Elbe and Saale, the latitude of Hannover and the valley of the middle Main. He is able to define this 'West-Germanic' province on the evidence of the archaeological material itself. Its northern boundary marches with that of the Chauci and Saxons; on the east lies the great Elbe-Germanic belt, with its distinctive Marcomannic extension through Bohemia;

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while on the south and west runs the Roman *Limes*, along whose line both native and Roman sites have done much for dating Germanic material by Roman associations. A large part of that material consists of pottery, still almost entirely hand-made. Von Usler's treatment of this uninviting but most important stuff is a fine example of systematic thoroughness tempered by restraint before the temptations of the typologist. Its crude and ugly ornamentation, examined minutely in all its varieties, is shown up, especially by its contrast with the neat roulette-work beloved of the Elbe Germans, as a typically West-Germanic character; its shapes prove reducible to six basic forms, whose history and intermingling are skilfully assessed; and finally it leads us to the detection of a geographic division of the whole territory between distinct distribution-areas, each referable to a different tribal context. Over this basic pattern, native and imported Roman metalwork and other merchandise ranges widely; its description is followed by discussion, in which we turn from the geographical aspect to the chronological, to get the whole array of datable type-associations set out in tables, covering the period in archaeologically distinguishable phases. It thus becomes possible to draw up a chronology for the leading sites of habitation and burial, and separate sections follow on houses (rectangular commoner than oval), settlements (single steadings and villages both represented), and grave-types (various forms of cremation distinguished). The subject of trade is then treated in the light of all the foregoing (with reference to Mrs Brogan on Romano-German commerce in *J. R. S.* xxvi, 195 ff.), short-range trade among the comparatively dense population along the Roman frontier, long-range mainly following big rivers like the Weser. With geography and chronology thus each reduced to terms, the author finally returns, before ending with his 88 closely-printed pages of inventory and index, to the bearing of all this upon the history and identity of the individual tribes composing the West-Germanic people. After three admirable pages (173-5) on reasons for caution over harmonizing archaeological distributions with a tribal map of Germany compiled from Roman literary sources, the positive evidence is summarized, with the pottery as a determining factor. To put it briefly, the Hermunduri, Chatti, and Cherusci are each made to show a distinct cultural individuality, intermediate between that of the Elbe Germans and the populations between the Weser and the Lower Rhine. But the distribution of the Elbe-Germanic culture reveals its authors as Tacitus' Herminones, while the Weser-Lower Rhine culture must likewise be ascribed to his Istaevones. But these names, with that of the Ingaevones who comprised the Chauci and their neighbours along the North Sea coast, are not those of tribes, but of religious confederacies of tribes. And it therefore emerges that while in some cases (Hermunduri, etc.) tribes are recognizable by their own traits of material culture, in

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others the smallest unit so recognizable is a confederacy of numerous different tribes, whose bond was one of religion, centred upon a sacred shrine like the Istaevones' temple of Tanfana. It is of course possible that archaeological refinements will presently enable component tribes within the Istaevones to be more clearly distinguished, like the Chauci among the Ingaevones ; but it will remain none the less true that what, if he had no literary evidence at hand, the archaeologist would treat as directly comparable cultures, may turn out to be the product now of single large tribes, now of multiple-tribe religious confederations. Here in fact is a clear warning against the too ready application of modern political or racial categories to a past known only from its material remains. In this, as in much else that one could mention, von Uslar reveals not only his learning but his judgment. His book should have a long-lived influence over Germanic archaeology, and it is not without its lessons also for the study of our own Iron Age in the British Isles.

CHRISTOPHER HAWKES.

NEW INDIAN ANTIQUARY ; a monthly Journal of Oriental Research in Archaeology, Art, Epigraphy, Ethnology, Folklore, Geography, History, Languages, Linguistics, Literature, Numismatics, Philosophy, Religion and all subjects connected with Indology. No. 1, April 1938, pp. iv, 82 ; No. 2, May 1938 ; pp. 83-146, 2 plates. *Edited by S. M. KATRE, and P. K. GODE. Bombay, Karnatak Publishing House. Annual subscription (India), Rs. 12 ; (Foreign) 8 dollars or 25 shillings, inclusive of postage.*

Archaeologists in this country would welcome an up-to-date journal that would keep them informed of the state of research in India and of important new discoveries that may be made. Unfortunately the *New Indian Antiquary* seems likely to disappoint them. The only article in the first number which is at all intelligible to the reviewer is one on Southern India, Arabia and Africa, which consists largely of quotations and summaries of European writers. Even these are carelessly set down ; for instance, an extract from the Arab writer Ibn al Mujawir confuses the text of the Arab writer with the words of his French translator, attributing the latter's statements to the former. The other articles deal with the Buddhist literature of Bengal, Parāmarthasāra, notes on the Katha Upanisad, Schopenhauer and India (!), the Bhagvadgītā riddle, Avijnaptirupā,* the Gaudi Riti (which appears to be roughly equivalent to ' gaudy writing '), a Hallmark of Man and of Religion (completely unintelligible), and Antaraghara. The greater part of all these articles is concerned either with the meaning of

* This is described in the title as ' (concealed form of activity) '. It is said to be a form of ' physical action ' occupying the interval between a promise and its fulfilment. Many of those who are kind enough to write for ANTIQUITY, for instance, unconsciously practise Avijnaptirupā, sometimes for months on end.

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words or with the discussion of philosophical and religious ideas. Those who like this sort of gibberish will find plenty of it here ; those who want to know something about Indian Archaeology, Art, History, etc., will put the book down in despair of ever getting any sense out of India. O.G.S.C.

PROSYMNA, the Helladic Settlement preceding the Argive Heraeum. By CARL W. BLEGEN. *Cambridge University Press*, 1937. 2 vols., pp. xxvi, 486, with 8 plates in colour, 731 illustrations, and 52 plans. £7 7s.

Prosymna according to Pausanias was the name of the district immediately below the Heraeum of Argos which stood on Mount Euboea well to the north-east of the city. It was discovered and tentatively excavated in 1836 by General Gordon of Cairness. Rhangabé in 1854 in further exploration of the site discovered the second temple of the late fifth century. In 1878 the beehive tomb which lies to the west of the Heraeum on the Mycenae road was excavated by Stamatakes. In 1892-95 the American School under Walston cleared the whole sanctuary, the foundations of the old temple on its cyclopean terrace wall, and the second temple on the terrace below with its colonnades and dependencies. Fourteen years later Friedländer argued from the beehive tomb and other prehistoric finds that the Hera shrine had developed on the ruins of a prehistoric town dating from Mycenaean times, if not earlier, which was probably the original Prosymna. The Heraeum stands like Mycenae on an isolated rocky hill between two streams and is protected on three sides by steep cliffs. The approach to the temple terraces rises gradually from the south. It has thus all the main characteristics of a prehistoric acropolis—ready access to water, a fertile region below, and a site easy to defend. Dr Hoppin, who had worked with Walston, was anxious to resume the excavations to test Friedländer's arguments and clear up the prehistoric antecedents of the classical shrine. He was prevented from realizing his wish first by the war and secondly by serious illness which ultimately proved fatal. Before his death in 1925 he had, however, made provision for the new work which was carried out for the American School by Professor Blegen in 1925, 1927 and 1928. The splendid publication before us is a proof of Hoppin's foresight and a worthy tribute to his memory.

Hoppin had two main objects in view (1) further exploration of the temple acropolis and (2) a search for prehistoric tombs. The temple terraces were proved to have been part of a prehistoric settlement dating from the dawn of the Bronze Age. No sign of a palace or great house was found and if such existed its ruins probably lie beneath the massive foundations of the old temple. The cyclopean supporting-wall, however, proved not to be older than the Early Iron Age. East of the second temple an altar like those at Delphi, Nemea, and Sparta was found which seems to be older than the temple. On the summit of the

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acropolis above the temple terraces habitation-remains of all three stages of the Bronze Age were found and so Friedländer's arguments were justified.

West of the site many tombs were found and during their excavation several neolithic deposits came to light. Some seem to be burials, but none a habitation. These belong to the first and second neolithic periods and contained pottery like that found elsewhere in Greece, notably in Thessaly. Of the tombs 32 belong to the Middle Helladic Period, one of the largest groups yet found in the Peloponnese and similar to the usual Middle Bronze Age tombs (*Symbolae Osloenses*, IX, p. 28 ff.). The contents of the tombs throw fresh light on rather an obscure period, and in one case the discovery of vases above the tomb may explain the scarcity of offerings in the tombs themselves. The fifty Late Bronze Age chamber-tombs fall into separate groups like the similar tombs at Mycenae from which Tsountas deduced that each group belonged to a separate clan. Each family seems to have had its own tomb which was used for successive generations, so that the older tombs show long sequences of interments running from L.H. I to L.H. III (c. 1600-1200 B.C.), and proving, if proof is needed, that there was no violent racial change during that period. The continuity of the civilization they represent is decisive, and is admirably illustrated by the vases, over a thousand, from the tombs. There are fine specimens of the early ware with lustrous paint which covers the transition from the Middle to the Late Bronze Age, some excellent L.H. I and II vases and a long series covering most styles of L.H. III. The other funerary gear in bronze, ivory, stone, and terracotta fills out the picture and shows that Prosymna was well supplied with all necessities and many luxuries.

This summarizes his record of the more important new material. An excavation, if it is not properly recorded and published, is mere destruction of irreplaceable evidence. It is comparable to the loss of a unique manuscript. Only too often in referring to old excavation reports do we find that the excavator omitted to record the presence or absence of some simple thing which later experience has shown to be a valuable criterion. An excavation report of this kind then must be judged in two ways, firstly as a description of the actual excavation and the facts observed which show whether it was scientifically conducted, and secondly as an interpretation of the material found which shows the archaeological capacity of the author. In both aspects this book is entirely satisfactory and achieves the highest standard of modern scientific archaeological work. This was to be expected from Professor Blegen, for he is not only one of the most competent excavators of the present day, but also one of the select band of scholars who have a deep and sound knowledge of Aegean archaeology, especially as regards Mainland Greece. Some critics (*e.g. Times Lit. Supp.* 1938, p. 204) say that in this book there is too much detail, that it is unnecessary

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to give plans and descriptions of all the tombs and their contents, that this report is an example of self discipline and over conscientiousness on the part of the excavator in the fulness of his records, that we have had enough of pottery and other objects of this kind and period. Such criticism is unbalanced and betrays ignorance of the subject. To publish a selection amounts practically to propaganda, for the author unconsciously but naturally enough is apt to select only what suits his theories. In any case this is the first Mycenaean cemetery outside Mycenae which has been properly excavated, recorded, and published. Had earlier excavators done their duty some of this might have been superfluous, but as they did not it is invaluable for the study of a prehistoric period where in Schliemann's words 'every potsherd is a new page of history'. This is a clear straightforward record of archaeological facts and every word of the commentary can be tested by reference to the descriptions and plans. As an account of prehistoric burial customs and culture, and as a chronological study of a valuable series of vases covering a critical period, this book is absolutely essential for the study of the Mycenaean age and as such can never be superseded. When even a work of the compass of Professor Myres' *Who were the Greeks* with its 634 pages published only in 1930 is already becoming out of date and must inevitably become quite antiquated, a scientific excavation report of this quality retains its value permanently.

The book is admirably produced. The arrangement of the text, description and commentary, and the illustrations is excellent, as well as the typography and the reproductions, especially the coloured plates which represent colour and texture extremely well. Last, but far from least, a thorough index makes the whole immediately accessible. The book deserves to be judged by the severest standards and passes the test with flying colours. A. J. B. WACE.

FRÜHGESCHICHTLICHE FUNDE AUS WESTFALEN im Städtischen Kunst- und Gewerbemuseum Dortmund. By CHRISTOPH ALBRECHT. *Veröffentlichungen aus dem Städtischen Kunst- und Gewerbemuseum Dortmund, Band 1*, 1936. pp. 63, 14 plates, 39 text-figures.

This volume is the first of a series in which the collections in the Dortmund Museum are being published. An authoritative account of the finds belonging to the Roman period, the first four centuries of our era, such as Dr Albrecht has here produced, was particularly to be desired, for the collection has accumulated during a half-century and much of it comes from sites inaccessible to further investigation owing to the industrialization of the region. Furthermore, the study of the west Germans has, until recently, lagged behind the study of the east Germans. Varus and his legions have been strong red herrings in Westphalia and the neighbouring territory, drawing off energy and enthusiasm that

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might have been engaged in less exciting but more fruitful pursuits and, as Dr Albrecht says in his introduction, we still know very little of what became of the Cherusci or the Chatti, or how the great confederation of the Franks arose. His own labours, and those of other scholars like Rafael von Uslar, are now doing for the archaeology of the west Germans what, earlier, Kossinna and his school did for the east Germans.

The main part of the volume is taken up with the description of the finds, in alphabetical order of the towns or villages whence they came; among them are the contents of the important cemeteries of Rünthe and Veltheim, representing respectively the earlier and later parts of the period. There are plans of the cemeteries, a number of small maps, and ample illustrations. The majority of the finds come from burials, which are all cremations buried in urns or small pits, and not marked by mounds. Grave goods are not very common, most offerings having been burnt on the pyre, but enough have been preserved to permit the author to draw some general conclusions as to the life of the tribesmen who owned them. Dr Albrecht pictures a flourishing population, definitely German in culture, and outside serious Roman influence, but nevertheless trading freely with the civilization across the Rhine and accumulating Roman money as a means of exchange. He refers to a house of the period which was excavated at Kamen and in the foundations of which a hoard of Roman coins turned up, and he believes that many other hoards must have come from such dwellings of which the traces had disappeared or remained unnoticed. The most notable Roman objects listed are Samian bowls (Dr. 37, from Trier), lamps, a glass urn, and a large number of bronze vessels, particularly Hemmoor pails, which in the third century, as at Veltheim, were frequently used as burial urns. In the third and fourth centuries the widespread east German influence made itself felt, and neck rings of east German type as well as brooches reflect this. The most important class of German material is the pottery, of which too little has hitherto been familiar from Westphalian sites, and the series of urns from Rünthe is specially valuable.

OLWEN BROGAN.

CORPUS VASORUM ANTIQUORUM: Yougoslavie, Musée de Prince Paul, Belgrade. By N. VULIĆ and M. GRBIĆ. Vol. 1. *Paris*, 1937.

This volume appears as a successor to the great collection of ancient ceramic, published by the Union Académique de Paris, which dealt chiefly with the finds from Vučedol in the Museum of Zagreb-Agram. Its subject is prehistoric ceramic down to and inclusive of the Hallstatt culture. The specialist in prehistory will be grateful both to publishers and editors for thus making known a series of important finds. There are 31 plates, from excellent photographs;

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each piece is described in detail, with information as to the circumstances of the finds so far as this is possible.

The editor gives as an introduction a brief chronological summary of the material. He divides the Neolithic period into four stylistic stages, and the quite separate 'Aeneolithic' into two, while the Bronze and Iron Ages fall into three divisions. This may be allowed as correct for the Stone Age finds, although a few observations are necessary. It is now generally recognized that the Starčevo culture, with its coarse and fine painted pottery, belongs to the earliest cultures of the lower Danube. The Vinča culture is next treated, of which the Belgrade Museum, in contrast to the University collection of Herr Vasić, possesses only a small selection, but a fairly comprehensive one. It is a matter for congratulation that another important settlement of this culture has now been brought to light at Aradac. The editor rightly leaves undecided the question whether a late stage of this culture is represented by the finds from Pločnik, marked as style c of the Neolithic period, and already published by Grbić. The other possibility is that they form a local variant, but neither as yet can be proved. Only a small part of the finds from Gradac, likewise assigned to this group, may actually belong to it, while the incised-ware sherds (plate XII, 1, 3-7) are to be assigned to another separate group, represented also in Banat. The Bronze Age is represented by two important collections belonging to the Bodrogkérésztur culture from Banat, Čoka, and Srpski Krstur. These afforded evidence both of an earlier stage (pl. XIV, cf. Tompa, *Berichten d. Röm.-Germ. Komm.* 24/25, group from Tisza-Polgar) and of the Bodrog culture proper. Grbić designates the Vučedol culture as style B of the Bronze Age. However, some of the finds illustrated as belonging here, from Zók in Hungary (pl. XVIII, 4, 5, probably also 1) do not belong to this but to the Baden culture, which often appears together with the Vučedol culture. Some other vessels also (pl. II, 15; XV, 8) may be assigned to the Baden Culture.

The division of the Bronze Age was a difficult task for the editors. The Jugo-slavian finds belong to the same culture-circle as the rich Bronze of southern Hungary, and can therefore be properly assessed only in close connexion with it. Unfortunately there is no comprehensive monograph on the Hungarian Bronze Age, combining in one synoptic account the evidence from the later settlements (Tószeg, Pecica), the comparative study of the finds from burial-places (*Depotfunde*) and their typological evaluation. For this reason, different views are held by recent writers, according to their method of treatment, on chronology, sequence of stages, and delimitation of cultures (Tompa, Childe, Åberg, Nestor). The earliest Bronze Age is so far but scantily represented in Jugo-slavia (pl. XIX, 1-3, probably also the vessels in pl. XX, 1, 2, ranged under style B). The Belgrade Museum is richer in ceramic from the

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later stages (pls. XIX-XXVII), which can be analyzed elsewhere and with greater accuracy than is attempted here, in connexion with the treatment of the Hungarian Bronze Age. Special mention must be made of the tankards with handles (pl. XXVII, 4 and 6), which show undoubted affinity with the Middle Danube (cf. K. Willvonseder, *Die mittlere Bronzezeit in Oesterreich*, fig. 176). The group from Dubovac, marked as an Early Iron Age culture (encrusted ceramic) may perhaps be older, and may even be assigned to the Bronze Age proper. The vessel shown in pl. XXXI, 8, ranked as style B of the Iron Age, but unfortunately of unknown provenance, is an important proof of the wanderings of the Lausitz urn-field culture. It is closely akin to the early types of finds from the great Slavonic burial-grounds at Dalj, Velika, Gorica, etc., and probably constitutes a link with the Lausitz elements which Childe has shown to exist in Greece (*Mannus Erg.* vol. 6), and to which in all probability belong also the finds, hitherto unpublished, from the rich burial-ground of Pateli in Macedonia (Istanbul Museum): these show a ceramic painted certainly in the southern style, but in form strongly reminiscent of the Lausitz type.

It is much to be hoped that the collection of the 'Corpus Vasorum Antiquorum' will soon include similar accounts of all the museums in southeast Europe; for up to the present time only such specialists as have been in the position to travel in those regions know what important and rich finds are often in evidence there.

WERNER BUTTLER.

SIR GRAFTON ELLIOT SMITH: a biographical record by his colleagues.

Edited by WARREN R. DAWSON. *Cape*, 1938. 12s 6d.

This is an interesting and readable account of an unusual career. Elliot Smith was primarily an anatomist, and all those qualified to judge are agreed that he was an anatomist of the first rank. But he is best known to the educated public as the Apostle of Diffusion. In the form in which he proclaimed it—and still more in the form in which it was proclaimed by some of his most enthusiastic disciples—Diffusion is not universally accepted by anthropologists and archaeologists. Our civilization has its roots in Sumeria and India as well as in Egypt; and the early cultures of South America cannot yet be satisfactorily explained by the theory of diffusion. It may ultimately be found that Elliot Smith's theory was partially right, though his facts were often wrong; time and further research alone can make this clear. What is certain is that Elliot Smith was a great originator, one who stimulated thought and revelled in argument.

It is largely as an anatomist that he appears in this biographical record, and we are glad that justice is done to his brilliant and original work in this branch of science. His work on the Neopallium and on the origin of the human brain was we venture to think, his greatest contribution to knowledge.



SIR GRAFTON ELLIOT SMITH (*see pp. 120-1*)
A photograph taken at Salmonsbury Camp, Gloucestershire, by O. G. S. Crawford

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Those who had the pleasure of knowing him personally recognized his genius, urbanity, and essential kindheartedness. A happy accident was responsible for securing a photograph which we publish here (see PLATE). It is right that his life should be recorded and the book here reviewed is both worthy of its subject and eminently readable. O.G.S.C.

A SURVEY OF PERSIAN ART. *Edited by* ARTHUR UPHAM POPE and PHYLLIS ACKERMAN. The publication will be completed in 3 vols. of text and 3 of plates, subscription price for the whole £36 15s *od.* Oxford University Press, 1938. Vol. I, text: Prehistoric, Achaemenid and Sasanian Periods, pp. 895. Vol. IV, 510 pages of plates of which 256 refer to Vol. I.

The volume under review comprises 38 principal and 13 subsidiary chapters; it fills 895 pages and is the work of 37 authors. It is impossible to do it full justice in a short notice, and primary attention will hence be given to the chapters dealing with prehistoric art, as likely to be of greatest interest to readers of *ANTIQUITY*.

After an introduction (Pope) dealing with the characteristics of Persian art as a whole, the work opens with a chapter by Herzfeld, entitled 'Iran as a Prehistoric Centre'. It is without doubt one of the most brilliant in the volume, and serves as a model of what chapters in such a composite work should be, for it is at the same time a synthesis of old and a presentation of new material. Herzfeld is of the opinion that agriculture first began on the plateaux, not in the lowlands, where irrigation presupposes a highly developed culture. In the Nearer East the main centre of development was the Caspian highland, where a distinctive culture-complex, characterized by such features as matriarchy and the serpent cult, was developed at an early date. The culture influenced the lowland areas around it, and the similarities which we see at Anau, in the Indus valley, in Mesopotamia, and in Elam are to be accounted for by this common origin. Various waves emanated from the centre at different periods, and it is to later ones that we owe the dissemination of metal-working, again first developed in the highlands, where raw materials were available. This time it was not only the East that was affected, for the 'prospector Folk' carried the new inventions to Europe. Once metals were introduced, however, the plain lands became the main centres of progress; cities were founded, writing was invented, and the highland regions soon assumed the position of conservative outposts. Herzfeld's theories are borne out by numerous facts—legends of a mountain home common among plain dwellers among them—and there seems every reason to accept his main thesis. The primary role which he assigns to the Caucasus and Anatolia may however be disputed by some.

Grousset's outline of the history of Persia which follows overlaps to some

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extent with the preceding chapter as regards the earlier ages ; for the later it affords a most useful summary, though it might have been more fully illustrated by sketch maps. It brings to bear what is rather a new light to most of us, for it is written from the Iranian rather than the Classical point of view. The originality and merits of Achaemenid culture are thus stressed, and the author shows that this age was not merely one of barbarism as compared to Greece. The unfamiliar Eastern aspect of the Alexandrian and post-Alexandrian periods is also clearly dealt with, and Grousset's conclusions are supported by data presented in a number of the chapters that follow.

In chapter 4, on 'The relations between Geography and Art in Iran'—a subject which should be dealt with in all works on the art of a particular region—Pope opens up a number of important lines of enquiry, but to many of them the answer seems at present to be the proverbial lemon. In actual fact but little has been done along this line of study, and it is to be hoped—and expected—that future researches will lead very much further.

Chapters 6, 7, 8 and 9, dealing respectively with 'the early cultures of Susa' (R. de Mecquenem), Damghan (Rogers Warren), Asterabad (F. R. Wulsin) and southeast Persia (Sir Aurel Stein), are all important, but there is certain repetition, and one chapter by a single author, presenting more of a synthesis, would have been more suitable for such a work as the Survey. The necessity of embracing a wide horizon is borne out by Contenau's admirably systematic and penetrating analysis of the ceramic wares of Susa, which follows. A comparative study of the material brings him to the conclusion that the stages at Susa should be redated in the order Susa 1 *bis*, Susa 1, Susa 2, and he insists that they constitute but an episode in the linked story of ceramic development in the whole Near East. He agrees with Herzfeld in assigning the first developments to an upland region.

Chapters 11, 'Cult Figurines' (Ackerman) and 12, 'Personal Ornaments in pre-Achaemenid Iran' (Przeworski) are in the main surveys of material ; chapter 13, on 'The Bronzes of Luristan' (Dussaud) is more penetrating, and presents a most useful and concise summary. This important material is divided into three main groups, assigned to the first half of the second millennium, to the second to first millennium, and to about the sixth century B.C. respectively. Dussaud concludes that the art, in the main Iranian, was inspired by Mesopotamia, and had very wide affinities. In spite of the mastery of treatment which the chapter shows, one feels that the last word on dating has not yet been spoken, and a sequence dating on purely stylistic grounds, which would permit closer assignments within the groups, should prove possible.

A short chapter on early seals in two parts (Ackerman and Rutten) follows, and the section closes with 'The pre-Achaemenid Animal Styles' by the two

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editors. The treatment is aesthetic rather than archaeological, and the ability to combine stylization, intensity, and vitality which is characteristic of Iranian art, is well brought out.

The most important chapters on Achaemenid art are those on architecture (Wachtsmuth) and sculpture (Casson); they both offer the only complete surveys that are available, and in both the individual, as opposed to the eclectic, nature of the art is stressed. For the Parthian age the chapter on Architecture (Reuther) is of the first importance. The author not only puts before us a great deal of unfamiliar material, but also presents a new synthesis of an important but neglected subject. When taken in conjunction with the same writer's brilliant chapter on Sasanian architecture, we see clearly that this bridge-period is of the first importance in the history of art as a whole, for a new spirit is born, which is later to become the very essence of the 'medieval'. Other chapters on Sasanian art which are important are those on metal-work (Orbeli) and textiles (Ackerman). The very high standard which the latter writer sets herself here is, unfortunately, not kept up in some of her other work, more especially in the chapter on the work of the Parthian silver- and gold-smiths. It is perhaps to be regretted that of the 51 full and subsidiary chapters Dr Ackerman had to be responsible for as many as 10, on the most widely diverse subjects.

The volume ends with a chapter on 'Some Problems of Early Iconography' (Ackerman), which deals with the well-nigh limitless topic of the origin of a number of the stylized motives so popular in Iranian art. Though nearly all the motives doubtless had a symbolic origin, their later repetition was, in the reviewer's opinion, more often due to conservatism than to deep-seated symbolic ideas, and the long chapter is thus at times something of a wild-goose chase. Its main theme, the necessity of taking into account the age-long history of motives in any attempt to explain later designs, cannot, however, be disputed.

The thanks of all specialists are due to the editors, publishers and sponsors of the 'Survey' for making an enquiry into the history of motives and styles in Persia practicable within the pages of a single book. The wealth of material of far wider nature that is presented is, in addition, of the very first importance to all archaeologists and art historians, whether specialists on Iran or not.

The volume is excellently edited and produced. The plates are of high quality, but at times one feels that less familiar material might have been illustrated.

D. TALBOT RICE.

PROFILES OF GREEK MOULDINGS. By LUCY T. SHOE. *Harvard University Press*, 1936. pp. 183, and 85 plates. Price not stated.

This massive work consists of a catalogue of all the most important architectural moulded forms from the points of view of their profiles. From the

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close study of the development of shape and form the author has established a morphological system for the systematic dating of Greek architecture. She concludes that by the middle of the 6th century B.C. Greek architects had developed from the two principal pre-Hellenic mouldings the seven main types which became the repertoire of all Greek building and of all subsequent architecture based upon it. No further development of these profile-forms is seen after the end of the 4th century B.C., when they were finally fixed after the changes of a fluid development. Thereafter all that happens is a re-shuffling of elements and forms, archaism and eclecticism.

This work will be of great value to architects and also to those who wish for archaeological datings. It is one of the major works sponsored by the American School of Classical Studies in Athens.

S. CASSON.

EARLY MAN: as depicted by leading authorities at the International Symposium, Academy of Natural Sciences, Philadelphia, March 1937. Edited by GEORGE GRANT MACCURDY. London: J. B. Lippincott and Co., 1937. pp. 362, 27 plates and 54 text-figures. 21s.

In March 1937 was convened at Philadelphia the first international symposium ever held on Early Man. It was a happy thought to make available in book form the contributions of thirty-six of the leading workers in the sciences which throw most light upon the earliest chapters in human history. The result is a thoroughly stimulating and worthwhile book. To review in detail the contributions therein contained would be tedious, and in view of the wide circulation which it deserves to achieve, superfluous. The occasion is rather one for reflection and general appraisal.

On putting down the book two impressions were uppermost in the reviewer's mind—a sense of the universality of human history and a realization of the intimate contacts of prehistory with the natural sciences. It is a commonplace that human history is world history, but it is true nevertheless that with the emergence of nationalities it becomes increasingly permissible and even essential that it should be studied in compartments. Already with the establishment of settled communities based upon the neolithic arts, soon to be fortified by the use of metal, regionalism tended to play an ever increasing role in the development of civilization. The days when it was possible to study even the palaeolithic cultures without taking due account of place have long since vanished; but it remains true that the palaeolithic provinces are comparatively large in extent and few in number. The study of the palaeolithic necessarily tends, therefore, to be a world study. The exigences of scholarship add point to the contrast. Whereas it remains possible for the student of the palaeolithic to acquire a reasonably exact knowledge of its manifestations all over the world, this

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becomes progressively more difficult for specialists of the later periods. The more settled the community the more differentiated and individual does its culture tend to become. In consequence the student of the later cultures is compelled, if he is really to master the details of his subject, to limit his range geographically. This goes far to explain the remarkable predominance among archaeologists of world-reputation of men whose lives have been devoted primarily to the palaeolithic. In this book the world-wide range of palaeolithic studies is reflected by contributions ranging over northwest Europe, the Near East, India, China, Manchuria, Java and South Africa.

Two of the main functions of the natural sciences in the field of prehistoric archaeology, namely the reconstruction of the physical environment of early man and the provision of a natural system of chronology, are well exemplified in chapters relating to pleistocene mammals and mollusca, profiles of weathering, pollen-analysis and geochronology.

A third feature of the book, which many people will find the most intriguing, is its attitude to the problem of the earliest settlement of man in the New World, a problem with which some fifteen contributions are directly concerned. Specific discoveries discussed in some detail include the original Folsom finds, the Vero find, the Confins Man and various other allegedly ancient human remains. Of the more general surveys of the evidence and inherent probabilities some are written mainly from the natural history angle—e.g. discussions of the geographical evolution of the Behring Straits, the pleistocene mammals of North America in relation to Eurasian forms, and the domesticated plants of the New World and their implications. Others are written more exclusively from the cultural standpoint, including brilliant contributions by Davidson on the question of Trans-Pacific Migrations, in which he delivers a slashing attack on the so-called Manchester school of diffusionists; by Spinden on the general question of the antiquity of man in North America, a powerful exposition of the conservative case; and an important one by Birket-Smith on the Eskimo cultures in their bearing on the prehistoric cultures of North America and Eurasia.

The only conclusion to which the more specifically American contributions lead one is that, although the association of the Folsom culture with certain types of mammal which in Europe would be regarded as of Pleistocene age is now widely accepted, and cannot reasonably be denied, the problem of the antiquity of man in North America is not thereby sensibly advanced: it remains to solve the problem of how long such mammals survived in this part of the world. The most promising lines of progress would appear to lie in the establishment of an indigenous natural chronology through the application in North America of geochronological methods, and in tracing successive waves of human migration between northeast Asia and North America. On the general question

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of the status of the earliest inhabitants of North America there is room for controversy. The hypothesis which the present reviewer ventures to advance is that man first arrived in the New World in a chronologically retarded Mesolithic stage of culture: that where suitable indigenous plants were available he domesticated them, and in favoured areas succeeded in building what can justifiably be termed 'higher cultures' on this basis; but that on the arctic fringe of the continent he maintained an ultimately mesolithic way of life up to the present day.

J. G. D. CLARK.

THE POTTERY OF PALESTINE: from the Earliest Times to the end of the Early Bronze Age. By G. ERNEST WRIGHT. American Schools of Oriental Research, New Haven, 1937. Publications of the Jerusalem School: Archaeology, volume I. *pp.* 106, *text-drawings and tables.* \$1.75.

This study marks a new departure in the publishing schemes of the American Schools of Oriental Research, but not only thus is it a novelty. The text is a photo-lithoprint reproduction of the author's manuscript, including drawings. Presumably this method was adopted to save expense, an object which seems to have been attained. While not as easy to read as a printed text, the typing is admirably clear, but the drawings and the lettering thereon are not so successful.

The survey is divided into three periods:—Neolithic; Chalcolithic: A, the Ghassulian Culture, B, the Esdraelon Culture; and the Early Bronze Age, I to IV.

The treatment of each section, very thorough and extremely fully documented, calls for very little comment. Wright follows Albright's chronological opinions, which is very satisfactory, and endorses that scholar's nomenclature for Middle Bronze I as against Fitzgerald's preference for E.B.III. The Chalcolithic is subdivided into sub-Chalcolithic, lower, middle and upper Chalcolithic, a conventional but neat and useful type of arrangement. An interesting and timely argument is made in favour of a development from Neolithic to Chalcolithic, in opposition to the theory of an influx of new people.

The study of the Early Bronze Age is possibly the most interesting and the most significant. The origin of the culture is pushed back from the conventional date of 3000 B.C. to the 32–31 centuries B.C. and the whole period is divided into four, with a further subdivision of E.B.I into A and B, which involves a slightly higher antiquity than has been supposed for certain finds such as Tomb A at Jericho. Wright has performed a great service to students of the period in demonstrating that its archaeological history is more complex than has been realized, a point which might be noted by others than those interested in Palestine. In discussing the origin of the E.B. culture Wright looks for a

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northern influence, and in this respect some of his parallels are controversial or incorrect. On page 67 the author follows Frankfort in believing that in Anatolia the red slip technique developed out of the black ware; Frankfort's great study was written some years ago, and consequently is not so reliable today. In note 59 to page 68 Cypriot parallels for plain ledge handles are quoted; actually the example from Gjerstad is a good E.C.I basin with horizontal loop handles, not lugs, and the others (B.M.C. Vases I part 2, C 1-2) are almost certainly degenerate forms of an ornate handle type which flourished in the last half of E.C.I, probably dating to an early phase of E.C.II. On the same page the reference to high handled cups in Cyprus loses its point when it is remembered that this shape is not earlier than E.C.II, which can hardly be as early as Wright places E.B.I in Palestine. The Early Cypriot bowl parallels quoted in paragraph 5 on the same page are by no means so simple as they seemed to Gjerstad over ten years ago. The vertical ear lug is common in E.C.I, but not in the form indicated—it is concave and usually fantastically so. The bowl shapes as shown in Wright's illustrations do not occur before E.C.II, and then in such a variety that as shapes they could be paralleled almost anywhere; they are not characteristic of any stage in E.C. unless the E.C.II to III black top hemispherical bowls with the knob lug on the rim are included. Then the question of the Ophel bowl is raised again; Gjerstad first identified this as Cypriot, but if the drawing in the Bulletin of the Palestine Museum, no. 3, is correct, the shape is very unusual for Cyprus, in fact so unusual as to be regarded with caution. When discussing the chronology of the Ophel tomb Gjerstad appears to forget that he has already cited a parallel for the vertical string hole in E.C.I even though Arpera tomb 103 *bis* seems an unusual group for that period. It is also curious that a rare type of this piercing should be found abroad, instead of a commoner type. In addition, as Gjerstad himself has pointed out in relation to Mycenaean imports into Cyprus, a pot must have either intrinsic artistic merits or valuable contents to be exported. It is hard to fulfil either condition in this case. On the strength of this bowl Wright suggests that Gjerstad's chronology is too low, but if Frankfort's classification of this piece is accepted the chronological difficulty is removed. Frankfort's difficulty with Gjerstad's chronology is less acute when it is realized that the Red Polished I to IV ceramic classification need not be chronologically interpreted—it is doubtful if Gjerstad ever meant it to be. There are pots classified as R.P.III which have a good E.C.I history, and some which cannot be attributed at all, as Sjoqvist found in dealing with Lapethos Tomb 303 A, pot 4 (S.C.E. volume 1).

When closely examined the parallels between Cyprus and Palestine in the E.B. are not convincing. At present there is no reason to doubt the essential

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accuracy of Gjerstad's absolute chronology to within half a century at its lower limit, but it is possible that the upper limit will have to be reduced from 3000 B.C. This means that any Southward movement affecting E.B.I in Palestine is not likely to have had any influence on the establishment of the Early Bronze Age in Cyprus. (Omitting the chronological question there is not material evidence of sufficient frequency to point to a connexion with the North in E.C.I). While the homogeneity of the Cypriot W.P.III to IV ware may be open to doubt, neither *in toto* nor *in parte* should either ware antedate E.C.III which should belong to the closing centuries of the third millennium. If the rather striking resemblances between the pottery of the Troad and E.C.II to III noted by most writers is accepted as having some value, then a Southward movement about the middle of the third millennium must be postulated, perhaps having some obscure reflection in Palestine in Khirbet Kerak ware.

This is one of the most instructive studies written recently on Palestinian archaeology, and while no study is timeless this should be of value for many years. If a second edition should become necessary the addition of some photographic plates to supplement the charts would add to the value, even at a higher price. There is no general index, but this is not missed.

J. R. STEWART.

Corrigendum

The price of Mrs J. M. Jocelyn's *Perseus*,
reviewed in the December number (page 498)
should have been given as 10s 6d.

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Editorial Notes

IN this number we publish an account of some remarkable discoveries in the hinterland of Aden, lying behind the mountains that fringe the southern coast of Arabia, now part of the Aden Protectorate. It has been explored by Europeans who have brought back drawings and squeezes, and, more recently, photographs, and the results of excavations. Modern accounts will be found in Rāthgens und von Wissman, 'Sudarabienreise', and in Miss Freya Stark's 'Seen in the Hadhramaut', concerning districts to the north and east which are closely related to the sites described here.



Southern Arabia before Islamic times was divided into warring kingdoms, but the cities were wealthy and developed an interesting civilization which lasted from about 800 B.C. to A.D. 600. The land therefore is not without a history. The Sabaeans, the men of Qataban and Dhu Raidan, were literate when our own ancestors were still in the prehistoric period; and their country is celebrated as the realm of the Queen of Sheba. Men like Halevy and Glaser and Wyman Bury have given accounts of sites which must be considered with those here recorded by Mr Stewart Perowne (pp. 133-7).



During the winter of 1937-38 an English expedition consisting of Miss Gertrude Caton Thompson, now President of the Prehistoric Society, and Miss Gardner, went out with Miss Freya Stark and

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directed the first scientific excavation ever carried out in the Hadhramaut (see *Asia*, April 1939). Not only did they find and excavate remains of the historical period (dating to somewhere in the second half of the first millennium B.C.), but they also found abundant remains of the palaeolithic period (flint implements and suchlike). These are found under such favourable conditions that eventually it should be possible to elaborate a chronological scheme, and to determine the successive stages of human development that occurred there. (This has already been achieved in rough outline by the work of British, French and American students in Palestine, especially by the excavation of the Carmel caves, and it is an achievement of which we may well be proud).



In addition, then, to sites such as 'Im'adiya and Nuqub, there are others where important remains are in great and growing danger of spoliation. Unless some sort of supervision is immediately forthcoming, no one acquainted with the circumstances can have any doubt about what will happen. Ignorance of their importance and of the urgency of the task cannot excuse the neglect of a simple administrative matter such as this.



All the work done so far to describe, record, excavate and preserve the antiquities of the Protectorate has been carried out by the enterprise of private individuals. Not only have they and their supporters at home (individuals, be it noted, not in this instance Societies) made themselves responsible for the field-work in Arabia itself, but they will probably have to subsidize the publication of their results. As for the housing of the antiquities found, there is a museum of a kind at Aden, but it owes all its merits to the enthusiasm and public spirit of an amateur who has now left the Protectorate.

What has the Government been doing all this time? The answer is that it has done nothing at all except allow a volunteer to do his best with the museum at his own expense.



It is only fair to state that Mr Harold Ingrams, the British Resident in the Hadhramaut, has a very lively sense of responsibility for the antiquities in his province. Not only has he, personally and on his

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own initiative, made arrangements for housing some of them at Mukalla (inscriptions torn from their context, etc.), but he has also drafted some excellent antiquity laws based on those of other countries. These must however remain ineffective unless followed up by the appointment of a Director of Antiquities to enforce them. He has also earned the gratitude of archaeologists by saving the Hadhramaut from at least three pseudo-archaeological expeditions. (Mr Ingrams is already known to many of us from his articles on the Hadhramaut published in the *Geographical Journal* and *Journal of the Royal Central Asian Society*, the Lawrence Medal of which Society has recently been awarded jointly to him and his wife.



It is necessary that a Director of Antiquities for the Aden Protectorate should be appointed without delay, for now, when the Protectorate and the Hadhramaut is being first opened up, is the very time when the destruction of priceless antiquities will proceed most ruthlessly; and such destruction is irreparable, for there is no other record.

The measures that should be taken are :—

To protect known sites from spoliation.

To remove inscriptions from such sites to the safety of the Aden Museum. Neglect to do this *now* will lead inevitably to the destruction of invaluable texts which are as yet either unrecorded or only inadequately recorded.

The existing remains must be planned and recorded by an archaeologically trained architect before it is too late.

The contents of the Aden Museum need cataloguing, photographing and (so far as possible) publishing. Arrangements for the inscriptions involving no expense to the Government have been made; but more than that is required.



The principle of appointing a Director of Antiquities in such administrations is already recognized elsewhere—for instance in Malta, Cyprus, Palestine and Transjordan—and it was recognized in Egypt and Iraq where they were directly under British control. After meditating for a quarter of a century the Sudan government has at last appointed a Commissioner for Archaeology and Anthropology (Mr A. J. Arkell). It

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is the normal procedure in territories under the French, Italian, Greek and Turkish flags. Other countries which have come later within the orbit of western civilization, such as Iran for instance, stand high in the estimation of people of culture, largely on account of the regard paid to their antiquities. Some of the independent States of India have set a high standard in this respect. In Balkan countries the national monuments are sedulously cared for by properly trained archaeologists ; and the state even subsidizes the excavation and conservation of the more important sites. Only at the heart of the British Empire is it necessary, on each separate occasion when the need arises, to bring public opinion to bear upon lethargy. Nothing else obstructs the execution of a duty which is recognized in every responsible quarter. That the appointment of a whole-time fully trained Director of Antiquities for the Aden Protectorate, with sufficient funds for necessary work, would be welcomed by instructed opinion in this country, is certain. As Editors of ANTIQUITY, we think we can claim to know something of such opinion ; we could not have kept this journal afloat for twelve years if we had not had it behind us throughout. It is therefore, perhaps, not inappropriate that we should celebrate our 50th number with a demand that we are sure will receive the backing of all our readers.

'Im'adiya and Beihaṇ, Aden Protectorate

by STEWART PEROWNE

IM'ADIYA lies about eight thousand feet above sea level, on the plateau known as the *dhahir*, near the precipice, four thousand feet in height, which separates it from the plain on the southern or seaward side. It lies within the territory of the 'Audhala tribe, near the Yemen border. It is ninety miles from Aden as the crow flies, or four days by camel. The nearest inhabited centre is Mukeiras, from which it is about three hours distant on foot, to the east. At Mukeiras the Royal Air Force maintains a landing ground and a rest-house, and the Government has a wireless telegraph station there.

In November 1937, political duties, involving negotiations with the Yemen Government, kept me in Mukeiras for a week. The Royal Air Force authorities in Aden, who have always shown themselves ready to assist archaeological research, permitted Flying Officer Curry, who is an expert photographer, to join me and to make a thorough photographic survey of the site.

The ancient city lay upon and between two of the rugged hills that form the highest point of the whole plateau, and dominate the surroundings on all sides. The site was clearly chosen for strategic reasons. Not only did the fertile plain of the *dhahir* lie beneath its watch towers, but the main route from Aden to Beihaṇ and thence to Shabwa and Ma'rib, passed, as it still does, within a few miles of its walls, because then, as now, there is only one practicable path up the face of the precipice, so that travellers must perforce pass within patrolling distance of 'Im'adiya.

As to the name, 'Im'adiya, I was unable to find any local tradition. If it has been correctly transmitted—this is by no means certain among illiterate tribesmen—it may derive from the Arabic root, '*ad*', applied to old times, '*adiya*' meaning 'ancient'. 'Im', in the local dialect, would be equivalent to 'Umm', which is not infrequently found in Arabic place-names.

'Im'adiya being built on rock, the ruins are clearly visible, neither sand nor vegetation concealing them. They cover a wide area at many different levels. The crags and peaks must have supported towers and walls, while the hollow, forming a rough amphitheatre, seems to have contained the main buildings, these being protected by a wall across the valley. (PLATE I).

The whole site is strewn with blocks of stone, almost as though the

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town had been shaken to pieces by an earthquake, and the ruins left untouched ever since. There is some evidence that this did happen : for the entrance to the city from the southwest passed over a paved causeway (PLATE II), now rendered impassable by great masses of rock which have fallen upon it from above. Owing to the roughness of the ground, and the absence of any centre of habitation in the immediate vicinity it is unlikely that the ruins have ever been used as a quarry.

But there are signs that the city was deliberately damaged by human agency. A large stele, bearing on one side a snake in high relief, was found broken into pieces, of which two were lying at a distance of some forty paces from each other. A third portion, needed to complete the snake, was not found. The stele was presumably a cult object, which offended an iconoclastic conqueror. In places the walls show signs of having been breached and repaired.

Little beyond the heaps of stones remains of the buildings themselves, though here and there a course or two of masonry survives to show the plan of what must have been at least four buildings of considerable size and dignity. A few steps of a stone staircase suggest, by their shallowness and breadth, a sense of comfort in those who caused it to be built.

The finest masonry consists of smooth-faced granite blocks, arranged with the 'knife-edge' fitting which was used in Herodian and later buildings in Syria. The stones are, however, far smaller than those at Hebron, Jerusalem or Ba'albek, perhaps owing to the difficulty of transporting them over such rugged country. The face of the largest stone seen was about four feet by eighteen inches. The quarry was not found ; perhaps the causeway led from it. Two of the blocks in one building bear inscriptions, each of four letters, beautifully executed in high relief, the letters being probably the largest South Arabian characters known. The words are, according to Professor Ryckmans of Louvain, of magical significance, WD'B and YK'N (PLATE III). I found only one other inscription *in situ*, but it was so faintly incised that it could not be copied or photographed.

There is a cistern on the site with a carefully devised system of supply-conduits. Whether this is contemporary with the buildings I cannot say. I neither found nor heard of any well.

I obtained on the site two stones bearing inscriptions which are now in the Aden Museum. One, of marble, seems to have been an altar-top. The votive inscription, in the Qatabanian dialect, Professor Ryckmans tentatively ascribes to the fifth or sixth century A.D.

'IM'ADIYA AND BEIHAN

The site generally called 'Beihan' lies near the village of Nuqub, three thousand feet above sea level, about ten miles northeast of the town of Beihan and but a few miles from the point where the wadi of the same name debouches into the desert. It is about five days journey by camel from 'Im'adiya and nine from Aden, from which it is distant about 130 miles as the crow flies.

The ruler of the territory is the Sharif of Beihan, or rather his regent, he being a minor. A neighbouring village is occupied by Sayyids; on the side towards the desert lie the Bal-Harith Arabs, who are—or were—on friendly terms with the Sharif. I visited Nuqub in May 1938, and again Flying Officer Curry was permitted to accompany me. A thorough photographic survey was made both on the ground and from the air. This was supplemented in December by Mr James Duncan, whose more prolonged and detailed observations are incorporated in the following notes. Beihan—for such, as one of the inscriptions found on the site records, was its name in antiquity—lies about half an hour by camel or horse down the wadi from the Nuqub landing ground. It is two days by camel from Ma'rib and four from Shabwa. Its importance was probably due to its position at the junction of the Aden route with the Shabwa-Ma'rib and Cana-Ma'rib routes, that is, the incense road running from the capital of the Hadhramaut to that of Saba. It would be possible, by going through the desert, to pass by Beihan; but if its rulers were hostile, any caravan that did so would be liable to a flank attack. Actually, it was probably always preferable to go via Beihan, because it is furnished with wells. In May 1938, it was reported that a force was collecting at Ma'rib, in Yemen territory, to march on and occupy Shabwa. Spies reported that, unless the expedition could pass through Beihan, which was out of the question, it would be impossible to reach Shabwa until new wells were dug near Harīb.

The site lies among sand-dunes which fill nearly all the space between the two ranges of mountains flanking the valley, at this point about two miles apart. The wadi itself is broad, sandy and smooth, with areas of cultivation on either side. The site crowns a little mound on the northeast bank of the wadi. It is oval in shape, the axes being about eight by five hundred yards. From the air, the line of the walls shows up very clearly, and the ground plan of numbers of buildings can be traced. The site is free of modern dwellings, except for one *dar*, with its outbuildings.

On the eastern side of the wadi, about three-quarters of a mile away, is a small, conical hill, which clearly served as a quarry for the

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builders of Beihan. The galleries from which the stone was hewn can easily be described.

The track from the landing ground leads to a gateway in the south-west corner of the walls. The masonry of the corners is clearly visible, also the actual entry. But the gate seems to be a patch-work, as it includes in its walls an inscribed stone, evidently taken from some other building. A two-line inscription in large characters is cut into the stones to the left of the door. Another inscribed stone lies in the doorway itself. All these three inscriptions were photographed.

It is not easy to tell the walls from the dunes; all alike are blanketed with sand. But from the declination of the ground and an occasional glimpse of masonry, it seems possible that considerable portions of the walls may be almost intact.

Of buildings within these walls there are traces on all sides, stones, fragments of walls, and corners. But of only one are there visible any substantial remains; two sides of a building whose walls were divided into bays, a style of architecture which is found also in a detached building in the wadi. Enough remains—the walls rise to six or eight feet—to show the type of masonry. The stones are of great size, far larger than anything observed at 'Im'adiya, and not squared, but fitted together with great skill. The same type of building is still used locally, where stone buildings of great height and strength are to be found in large numbers. Within this building Mr Duncan found heaps of ore, which leads him to believe that it was the mint.

Near this building is visible the upper eight feet of a stele, inscribed on all four sides. This was reported to have been 'seen' by G. Wyman Bury in 1909. It was photographed on both of the visits made in 1938.

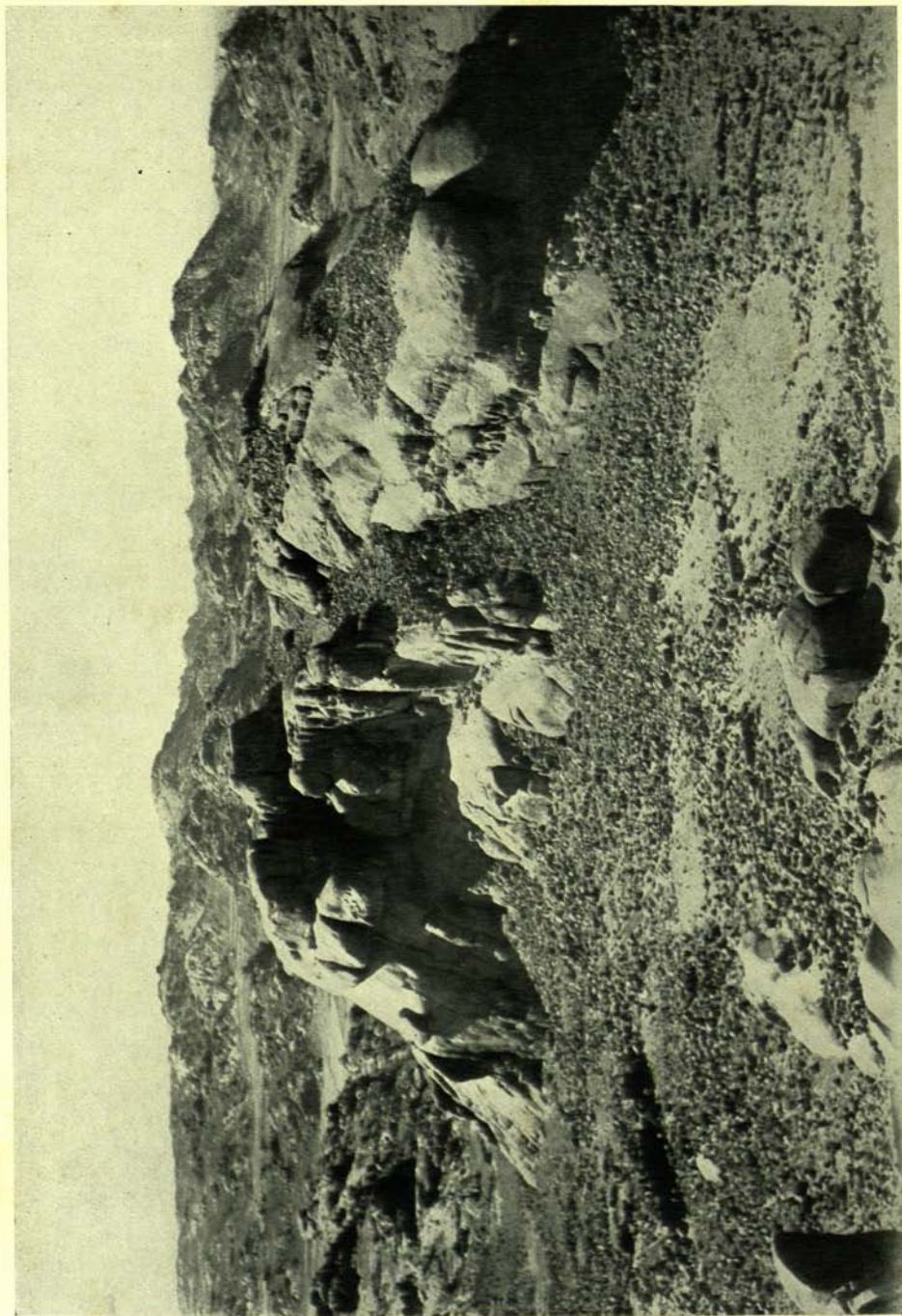
There are numerous wells, some within the confines of the ancient city and one in the wadi below, near the projecting outwork. Of the latter there remains a paved platform, at some twenty feet above the wadi bed, and behind part of a wall of the same type of masonry. There are also extensive traces of sun-dried brickwork in the vicinity.

The building is still called *al-Kanīsa*, the church, and seems to have been the temple to which the inscription mentioned below refers. It must have been a substantial and important edifice.

Mr Duncan also located the cemetery on a hill to the northeast. He examined a number of tombs, most of which had already been rifled. This cemetery is the reservoir of the alabaster plaques and statuettes that keep trickling into Aden.

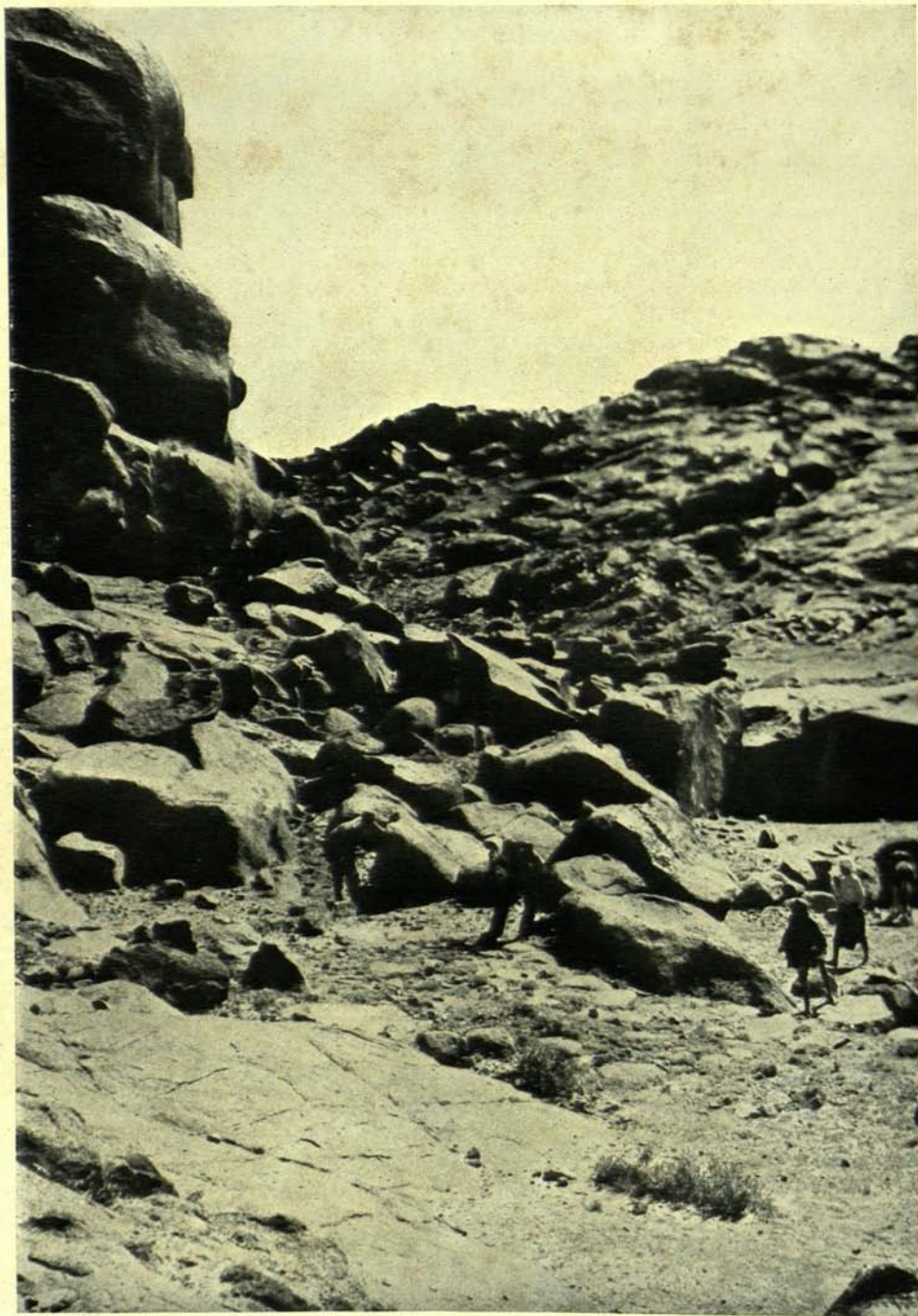
Some of the buildings within the walls were constructed of smaller

PLATE I



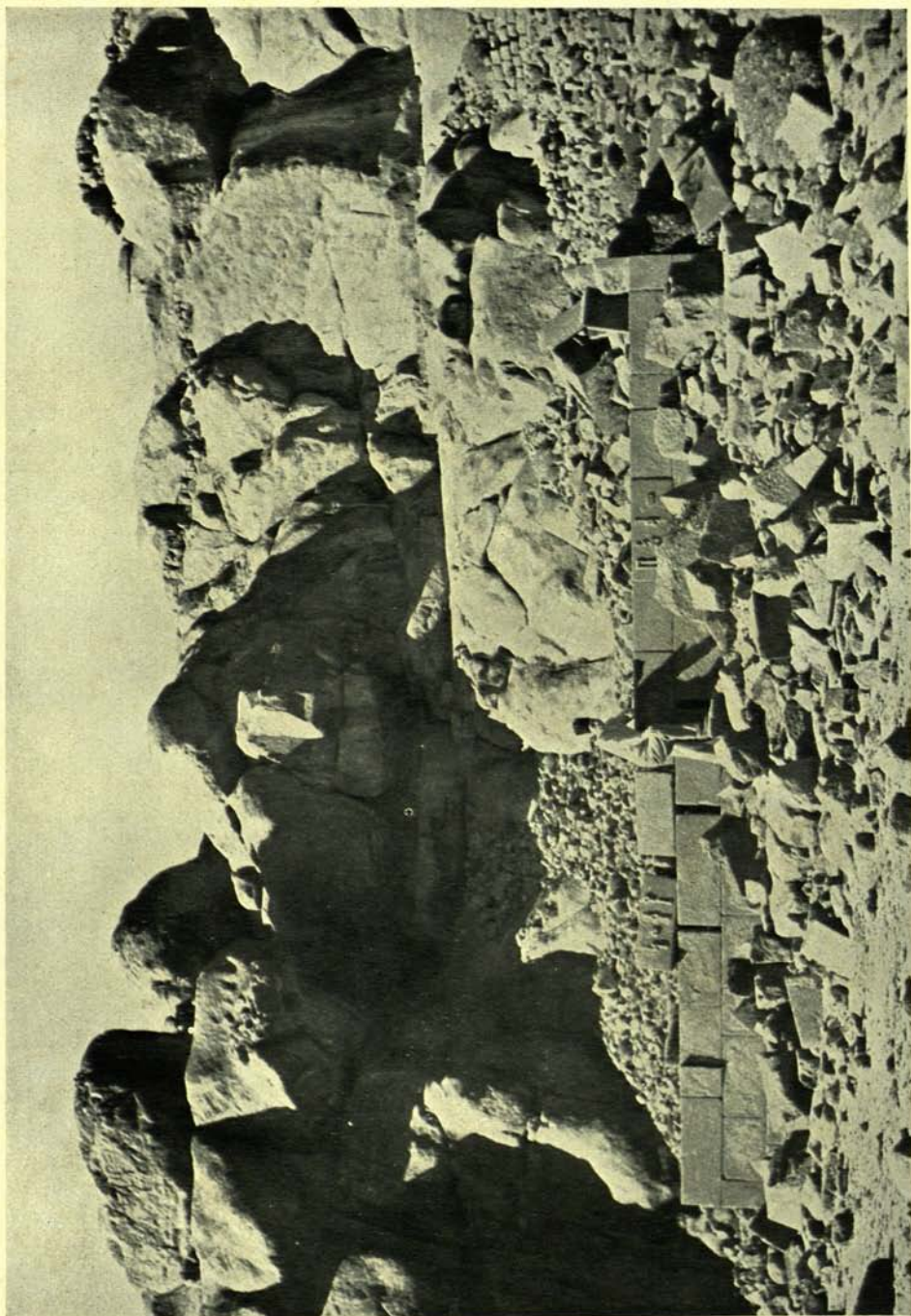
'IM'ADIYA, ADEN PROTECTORATE : GENERAL VIEW FROM THE NORTHEAST (see p. 133)

PLATE II



PAVED CAUSEWAY OF STONE SLABS LEADING TO THE ENTRANCE OF THE CITY OF 'IM'ADIYA
Now rendered impassable by masses of fallen rock (see p. 134)

PLATE III



GRANITE BLOCKS, 'M'ADIYA, SHOWING INSCRIPTIONS OF MAGICAL SIGNIFICANCE CUT IN HIGH RELIEF (see p. 134)

dressed stones and in one of them an alabaster box which was brought to me was said to have been found. It was of beautiful workmanship, a cube in external form, embellished with bulls' heads in high relief. It contained two circular compartments, rather like a dual inkpot stand, and was perhaps used to contain precious ointments, for which the region was famous (cf. Matthew xxvi, 7 and Mark xiv, 3). A gargoyle in the form of a bull's head, worked in stone, was also brought in. This object closely resembles the spout or gargoyle, also in the form of a bull's head, of an altar-top found by Miss Caton Thompson at Hureidha a few months earlier, and recently exhibited at the Fitzwilliam Museum, Cambridge. The most remarkable object which I had the good fortune to acquire was a little alabaster statuette of a woman or a beardless youth. This Professor Ryckmans has pronounced to be the most beautiful of its type which is known to him. It seems to display a Greco-Roman influence which, taken together with the style of masonry noticed at 'Im'adiya, suggests that the caravans which bore the spices of Arabia Felix to the Mediterranean brought back with them workmen who were conversant with Greco-Roman models.

These three finds, together with all other objects which Mr Duncan and I acquired at Beiha and 'Im'adiya, are now in the Aden Museum.

At the moment of writing, Mr Lawrence Kirwan, archaeologist to the Sudan Government, is making an inspection of the two sites. It is to be hoped that the results will throw more light on them and make possible a proper scientific investigation of sites which promise to be of first-class importance for the elucidation of problems which are still tantalizingly obscure.

The *dar* has built into it two inscriptions. According to Professor Ryckmans one is an abrupt invocation of the irrigation gods; the other records the rebuilding of the temple from bottom to top, to commemorate the overthrow of the people of the Hadramaut and 'Amrum by King Shahr Gaylan, believed by Professor Ryckmans to have lived in the 7th century B.C. In this inscription the name Beiha occurs. This mention of the name is of considerable importance, as it proves that this site cannot, as some have suggested, have been Tamna, which according to Strabo was the Qatabanian capital. One is tempted to inquire—if Beiha was not Tamna, could 'Im'adiya have been Tamna?

In the preparation of these notes I have received the greatest assistance and encouragement, both from Professor Ryckmans and from Mr Sidney Smith, the Director of the Egyptian and Assyrian Department of the British Museum, to whom I wish to record my grateful thanks.

Stone and Earth Circles in Dorset

by STUART AND C. M. PIGGOTT

THE primary purpose of this paper is to publish for the first time a complete set of plans and descriptions of the Dorset stone circles, and of certain allied monuments in which timber uprights may have played a part but whose visible remains today consist only of encircling banks and ditches. The existence of most of the sites described has been recorded, with full references, in the Ordnance Survey's *Map of Neolithic Wessex*, but we have been able to add several new circles to those listed there. The field-work was undertaken and the surveys made in 1936 and 1937, while in certain instances use has been made of air-photographs to which Mr O. G. S. Crawford has drawn our attention.

For once the county boundary, usually so uncomprisingly arbitrary with regard to prehistoric distributions, is a convenient definition of the area discussed, since the majority of the sites are concentrated in a small area in the Dorchester region, and the two outlying sites are both within the county—one in the Isle of Purbeck and one on the edge of Cranborne Chase. The general geographical setting is familiar from the Neolithic Wessex map referred to above; all the sites are on chalk with the exception of Rempstone, which lies on Bagshot Beds at the foot of a chalk ridge.

The types of monument represented divide, as the title of the paper suggests, into two main groups: free-standing stone circles and monuments in which a circular bank with internal ditch form the main and least destructible features. There is no example within our area of the combination of stone circle with bank and ditch in the Avebury-Arbor Low tradition.

STONE CIRCLES

As can be seen from the map (FIG. 1), these have a limited distribution on the chalk hills west of Dorchester, with a single isolated example at Rempstone in the Isle of Purbeck. Those of the Dorchester group are all built of sarsen of the local variety which embodies numerous flint nodules; that at Rempstone is of sandstone boulders

STONE AND EARTH CIRCLES IN DORSET

from the Bagshot Beds. This utilization of local stone is to be expected, but the absence of circles on the Purbeck limestone is curious. So far as we can see, the occurrence of stone circles in the chalk country is dictated by the presence or absence of sarsen—Stonehenge in this as in most other circumstances being a notable exception.

The typology of the Dorset stone circles is simple. They are all of small diameter, ranging from Kingston Russell's 80 feet to the 25

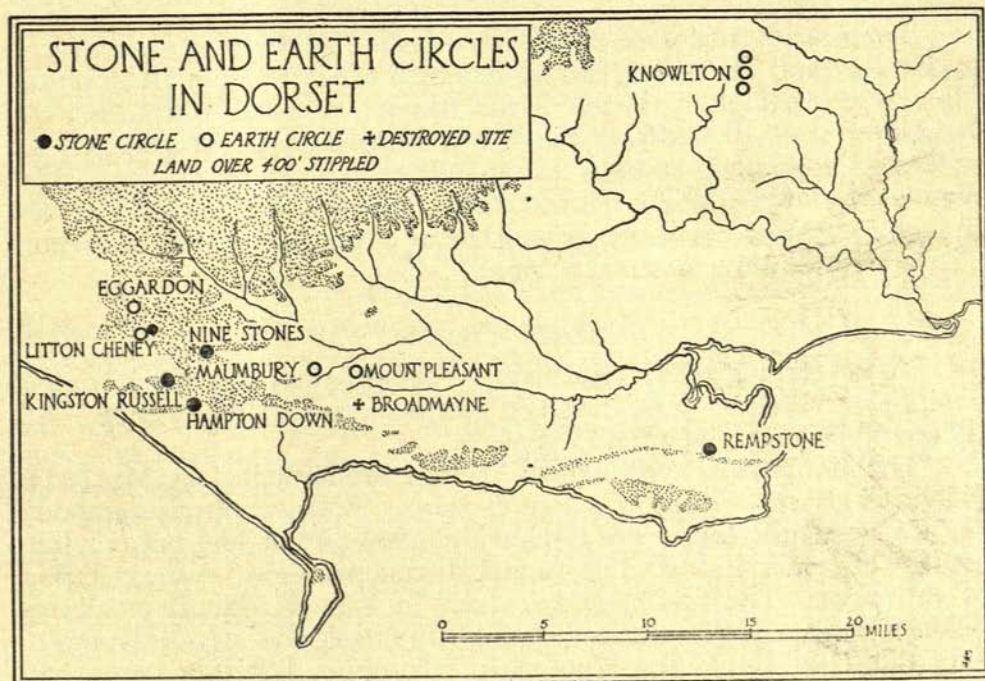


FIG. 1

feet of the Nine Stones, and none of the stones is large. No central stones or outliers appear to exist, and there is no trace of any earthwork associated with the stones save at one small and exceptional site, the smaller circle on the ridge above Litton Cheney, where a single stone remains on the edge of a very slight ditch. The adjacent circle, usually claimed as an earth circle, seems to us to be the remains of a stone circle in which orthostats, now vanished, stood in a low bank, analogous to Meini Gwyr and Penmaenmawr in Wales.¹

¹ W. F. Grimes, *Proc. Prehist. Soc.* II, 109.

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We have included two destroyed sites and a possible third, which seemed to have sufficient evidence to warrant their acceptance. The Little Mayne circle is of particular importance since that careful observer Roger Gale described it at the beginning of the eighteenth century as having the remains of avenues. Gale had worked with Stukeley at Avebury and his judgment must be respected, and Little Mayne must at least be given some consideration in the discussion of analogous sites.

The nearest analogies to the Dorset stone circles lie to the west—on Exmoor and Dartmoor, and to the north in the Stanton Drew group (the more particularly if the Little Mayne avenues really existed). It is interesting to find that the North Wiltshire type of stone circle, with two concentric settings (as the inner settings of Avebury, The Sanctuary and the Winterbourne Bassett circle) is not represented, although something like it occurs on Exmoor at the Almsworthy Common Circle, with its three concentric rings.²

EARTH CIRCLES

These form a more miscellaneous group than the stone circles, comprising as they do at least two distinct types of monument. All the examples recorded are on the chalk.

The monuments aptly named 'Henge Monuments' by Mr T. D. Kendrick are susceptible of a dual division. Both sub-groups embody the same essential features of ditch with exterior bank, and may contain settings of stones or posts, but we may distinguish them on the criterion of entrances. The first class, to which in Dorset Maumbury Rings belongs, have a single entrance through the bank and across the ditch. Into this class would come not only Maumbury but the Arminghall monument, Woodhenge, the earlier structure at Stonehenge, Gorsey Bigbury on Mendip and Mayburgh in Westmorland. The second class is represented by Durrington Walls (Wiltshire), Arbor Low (Derbyshire), King Arthur's Round Table in Cumberland and Thornborough and Ripon Moor in Yorks, and has two entrances opposed diametrically. Furthermore, as Mr Alexander Keiller points out to us, the single-entranced 'Henges' have this entrance to the northeast, while the double-entrance group have a northwest-southeast alignment.

Despite the remarkable features of its quarry-ditch, Maumbury Rings is a typical enough example of the first sub-group, while the

² H. St. G. Gray, *Proc. Somerset Arch. & Nat. Hist. Soc.* LXXVII, 78.

STONE AND EARTH CIRCLES IN DORSET

Knowlton circles may belong to the second, but without excavation it is impossible to define the positions of their entrances. The dubious site on Mount Pleasant should, from the position of the one identifiable entrance on the southeast, belong also to the second class.

At Knowlton, the barrow with its distant encircling ditch (as if an enormous disc-barrow) suggests yet another type of structure, and this is corroborated by the analogous site near Eggardon, which may have opposed entrances in normal alignment. It is conceivable that this is in some sense the prototype of the bell-barrow, and an intermediate form is provided by a barrow on the Ridgeway above Upwey, which has a disproportionately wide berm between it and its surrounding ditch. Neither Eggardon nor the Knowlton barrow-in-circle can however be dismissed as an abnormal disc-barrow—a possible comparison may rather be made with the Ysgeifiog circle in Flintshire.³

DATING EVIDENCE

None of the sites described has been excavated except Maumbury Rings, and here the evidence is not so conclusive as might be wished. The critical sherd of cordoned ware must however fall into the Groove-Ware series, and the flint-types are in complete agreement with an Early Bronze Age dating. The presence of the phallic representations carved in chalk may give a clue to some at least of the rites carried out in these sanctuaries.

Elsewhere we must rely on analogy, and one of us⁴ has elsewhere suggested that in the free-standing stone circles of Wessex we may see a 'Highland' culture which may be represented by the B1 beaker folk of Breton origin, and in the ditch-encircled 'Henges' a 'Lowland' tradition proper to the A beaker people from Holland and the Rhine mouth—the idea of the open circular temple being common to all sub-groups of Beaker folk. The groove-ware series certainly seems to have its immediate continental analogues in Holland, and its makers may well represent a slightly earlier move along the same routes as the Dutch beaker-folk.

But the Dorset stone circles need not all be of the same date. Mr Christopher Hawkes⁵ some years ago indicated the strength of the megalithic tradition in this region, which persisted in barrow construction until Deverel-Rimbury times. The so-called Pokeswell 'Circle'

³ C. Fox, *Archaeologia Cambrensis*, LXXXI, 48.

⁴ S. Piggott, *Proc. Prehist. Soc.*, IV, 37.

⁵ C. F. C. Hawkes in *Antiquaries Journal*, XIII, 433 ff.

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is a barrow with internal stone-setting, and similar structures explored by Warne and Cunnington yielded grave-goods of Middle to Late Bronze Age date.

Whatever may be the absolute dating, there seems every likelihood that the stone and earth circles represent distinct though allied strains, and that the series cannot be earlier than the Early Bronze Age in inception, although their use and possibly their construction may last into the Middle and even into the Late Bronze Age.

There is a class of monument which may represent the 'Henge' idea in some form during the Middle Bronze Age, represented in our area at the barrow group on the southern edge of Big Wood, Winterbourne Steepleton parish (6 in. o.s., Dorset XLVI, NE). Here is a small 'earth-circle', consisting of a bank 90 ft. in diameter, with no perceptible ditch, but with the entire inner area lower than the surrounding ground. It is in fact a 'pond-barrow'; a type known from Wiltshire and normally associated with a group of barrows, but of rare occurrence.⁶ Mr G. M. Young has noted seven Wiltshire examples, and has advanced an ingenious and plausible equation of these structures with the early Greek *βόθρος* or *χύτρος*, suggesting that they played a part in ceremonial libations and the evocation of ghosts from the underworld.⁷

Stone Circles

HAMPTON HILL (FIG. 2)

This circle lies in open downland above and to the west of Portesham at a height of 680 feet. The stones are of sarsen. The circle is incomplete, and a high hedge and bank run across from north to south, separating the three most westerly stones from the rest of the circle. Of the eastern half ten stones are probably in their original positions, suggesting an original diameter of 35 feet for the structure. The stones are spaced irregularly and owing to their rough cube-like shapes it is impossible to decide whether they are upright or recumbent, but most seem to be as upright as their shape allows.

Map of Neolithic Wessex, no. 143, with full references.

KINGSTON RUSSELL (FIG. 3)

This circle, which appears to retain its full number of stones, lies on open downland at a height of 620 feet, south of Kingston Russell.

⁶ M. E. Cunnington, *Introduction to Archaeology of Wilts* (1934), 87-88.

⁷ *ANTIQUITY*, 1934, VIII, 459-61.

STONE AND EARTH CIRCLES IN DORSET

It is sometimes referred to as the Gorwell circle from the farm of that name to the south.

All the stones, which are sarsen, are now recumbent, but in 1815 one stone to the south was still standing. Measuring from the fallen

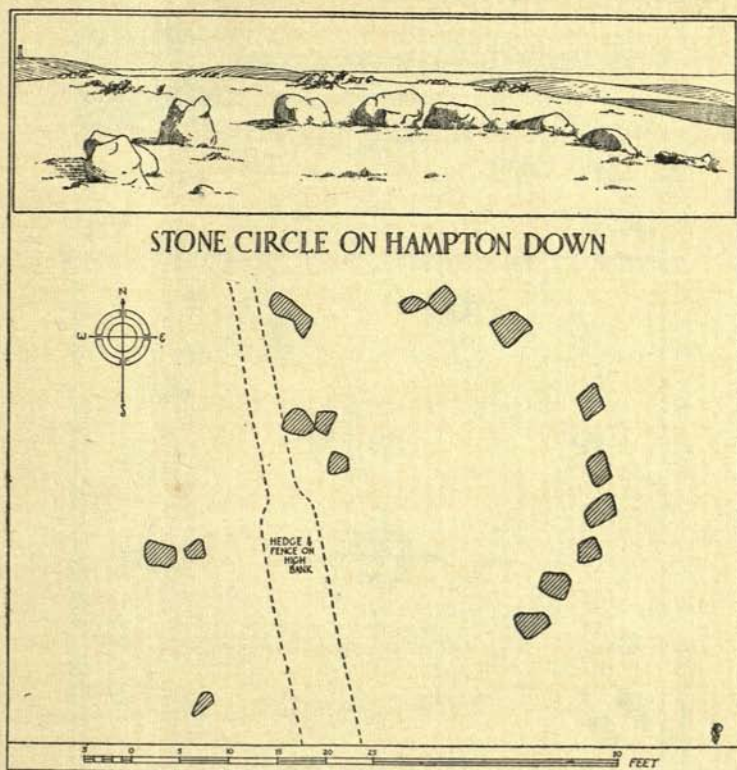


FIG. 2

stones the circle is 80 feet from north to south and 60 feet from east to west, but many of these may not be in their true positions.

Map of Neolithic Wessex, no. 141, with full references.

LITTON CHENEY NO. 1 (FIG. 4)

This small circle lies on the high ground north of the village of Little Cheney on the right hand side of the Dorchester-Bridport road. It consists of a shallow ditch with internal bank, enclosing a somewhat

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oval area measuring 75 feet from north to south, and 63 feet from east to west. The ditch, which dies out on the southeast, where the ground

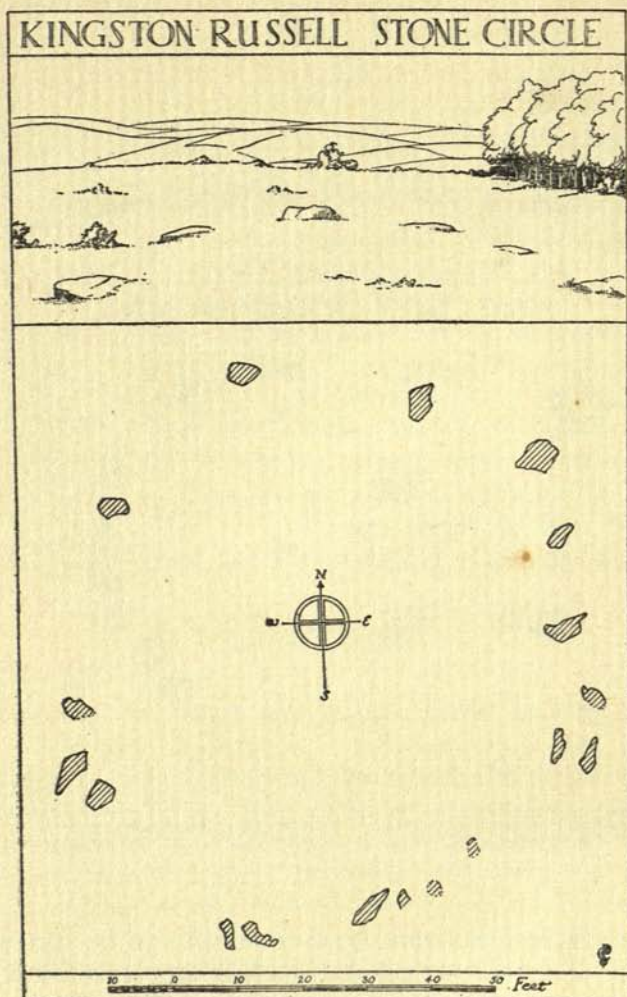


FIG. 3

has been disturbed, does not reach a depth of more than about one foot, while the bank rises nowhere above 2.5 feet.

It is possible that there was an entrance on the southeast but the bank is disturbed at this point.

STONE AND EARTH CIRCLES IN DORSET

On the crest of the bank on the southwest are three almost circular depressions, some six feet in diameter, and placed twenty feet distant from one another along the circumference of the bank. Another similar depression is on the northeast, while yet another may have existed in the disturbed portion of the bank on the southeast.

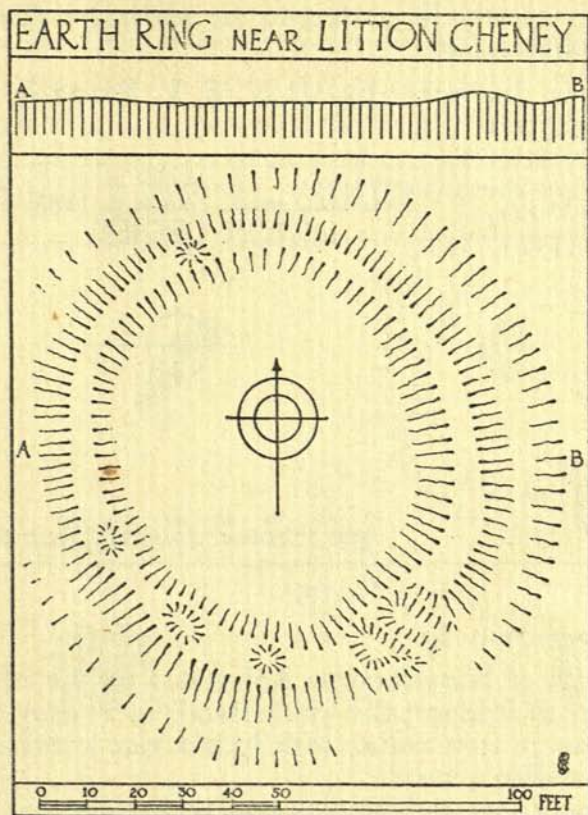


FIG. 4

These indications strongly suggest that this is not an earth circle in the true sense of the word, but that the depressions represent the sites of removed stones. The existing spaces of 20 feet between the depressions would give a total of twelve stones on the circumference of the bank, and as has been stated in the Introduction, analogies to such a circle exist in Wales.

Proc. Dorset Field Club, XXIX, 250.

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LITTON CHENEY NO. 2 (FIG. 5)

This site lies 136 feet from no. 1 at a bearing of 115° from its centre. It consists of a very shallow and regular ditch surrounding a circular area 47 feet in diameter. A single sarsen lies on the inner lip of the ditch on the southeast, and may be the sole survivor of a circle.

Three more sarsens lie 90 feet to the south, but their relation to the circle is problematical.

This site was discovered by Mr W. E. V. Young in the company of the writers in 1936.

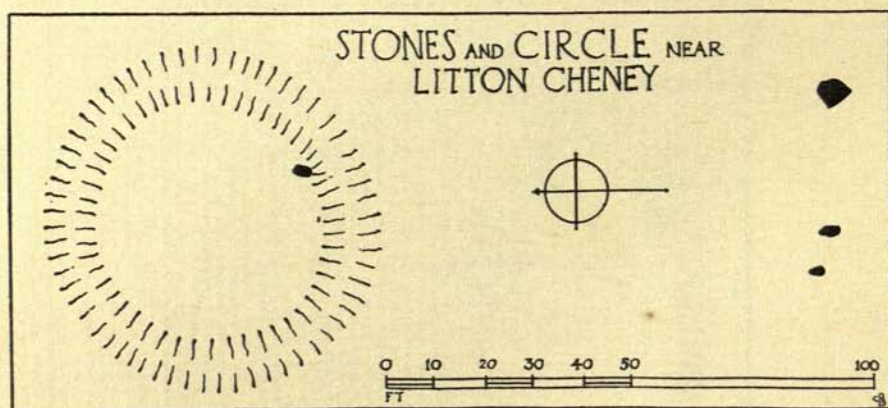


FIG. 5

THE NINE STONES (FIG. 6)

In the parish of Winterbourne Abbas and on the left of the road from Dorchester to Bridport this circle stands in a valley at a height of 340 feet. It has an iron railing with locked gate surrounding it, and stands in a clearing in a wood.

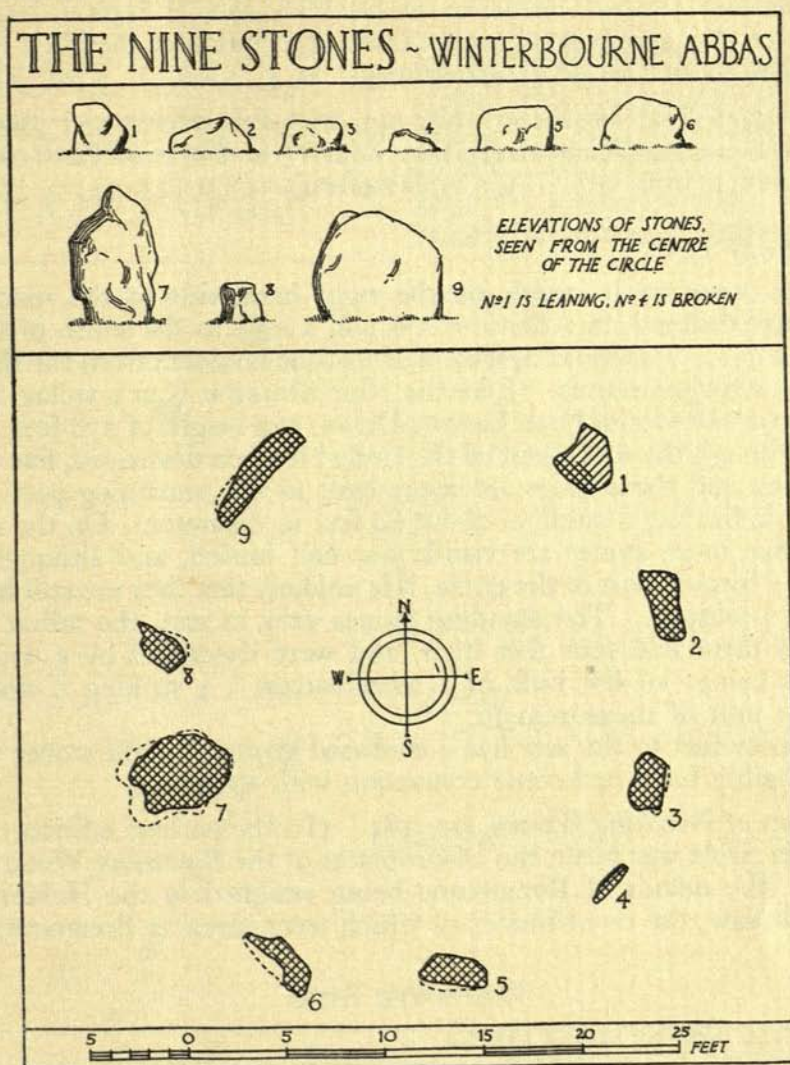
All the nine stones are standing and enclose an area of 25 feet in diameter; there may have originally been a tenth stone to the north as something seems to have been seen here by Warne. The extraordinary discrepancy in size between nos. 7 and 9 and the remainder of the circle is without parallel in our Dorset series.

Aubrey visited the site and records the existence of nine stones only, while the circle is shown in the same state as at present in Stukeley's drawing of 1723.⁸ This drawing, entitled 'A Celtic Temple at Winterburn' has since been wrongly identified by Mr O. G. S. Crawford

⁸ W. Stukeley, *Itinerarium Curiosum*, II, tab. 92.

STONE AND EARTH CIRCLES IN DORSET

as being that dug into by Dean Merewether in the parish of Winterbourn Monkton in North Wilts.⁹



Warne, who described the site in the nineteenth century, mentions a tenth stone 'which the eye detects just peeping through the long

⁹ O. G. S. Crawford, in *Wilts. Arch. Mag.*, XLII, 55.

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grass on the north-east side', but the writers' eyes were not able to detect more than nine in 1936. Warne still further confused the issue by supposing Aubrey's sketch of the Devil's Quoits at Stanton Harcourt, Oxon. (on the same page of his MS as the description of the Nine Stones) to have been another structure nearby.

Map of Neolithic Wessex, no. 149, with full references; two early XIXc. drawings in Wilts. Arch. Soc. Library at Devizes, Book N. f. 76. (Information from Mr O. G. S. Crawford).

REMPSTONE (FIG. 7)

This stone circle stands on the right hand side of the road from Corfe to Studland, in a dense wood half a mile to the south of Rempstone Lodge. Its stones consist of sandstone boulders from the Bagshot Beds on which it stands. Like the Nine Stones it is in a valley, in this instance at the foot of Nine Barrow Down, at a height of 278 feet.

Although the south half of the circle has been destroyed, five stones still stand and three more are recumbent in the remaining part of the setting, indicating a circle of about 80 feet in diameter. On the south-west three more stones are visible but half buried, and although they probably formed part of the circle, it is unlikely that they are still in their original positions. The standing stones vary in size, the tallest being between three and four feet high, and were described by a writer in 1900 as being 'of the bulk of a wheelbarrow', a striking if not very accurate unit of measurement.

Ninety feet to the east lies a confused group of eight stones which may possibly have had some connexion with the circle.

Map of Neolithic Wessex, no. 184. (In the earliest edition of this map this circle was given the co-ordinates of the Breamore Wood Long Barrow, the details of Rempstone being assigned to the Holdenhurst Long Barrow, the co-ordinates of which were given to Breamore).

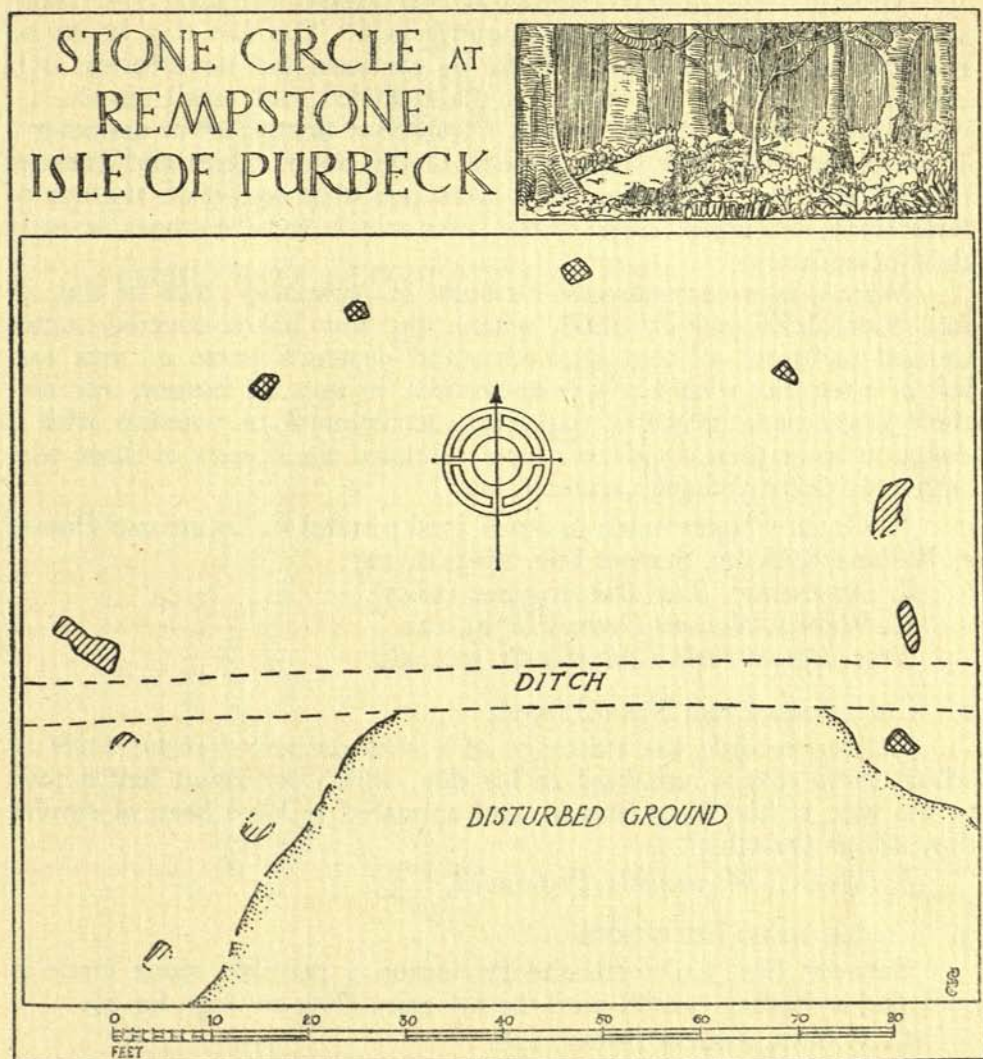
DESTROYED SITES

1. LITTLE MAYNE STONE CIRCLE

Omitted from the Neolithic Wessex Map, this circle, of which no recognizable remains exist today, is known to us through Warne, and two earlier writers whom he quotes. It apparently stood by the Dorchester-Wareham road, four miles from Dorchester, where today only a few scattered sarsens mark the site.

STONE AND EARTH CIRCLES IN DORSET

Roger Gale visited the site in 1728, but in his note upon it there is some confusion about the name (which he gives as Friar's Mayne), and



its distance from Dorchester. Warne however identified the site with that at Little Mayne. Gale records that there was 'a circle of stone lately broke to pieces by the owner of the land', and in addition he says

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that there were 'two avenues of pitch Stones leading up to it, one from the south, the other from the east'.

In 1847 Little Mayne received the attention of Mr John Sydenham, and was described in his *Baal Durotrigensis*. The fact that he styled it a Dracontium would alone make us cautious, but nevertheless it is difficult to discount his record of 'a complete but small circle . . . composed of ten or eleven stones . . . about thirty feet in diameter'. It seems probable that some at least of the outer circles and avenues that he goes on to describe were more probably seen with the eye of faith alone, and indeed some of the latter may have been stones on early field boundaries.

Warne, in a characteristic outburst of pomposity, tells us that all had been destroyed by 1868, when 'the few lichen-covered stones around me were all that ignorance and destructiveness of men had left of a temple, within whose enclosures, in ages far beyond our ken, their forefathers, perhaps, had been accustomed to worship with a devotion truer than could ever have animated the breasts of those who ruthlessly destroyed such a sacred fane!'

R. GALE. Notes made in April 1728 printed in *Letters and Diaries of William Stukeley*, Surtees Soc. 1883, II, 127.

J. SYDENHAM, *Baal Durotrigensis* (1847).

C. WARNE, *Ancient Dorset* (1872), 121.

Proc. Dorset Field Club (1909), xxx, xlv.

2. CIRCLE NEAR THE NINE STONES

Aubrey records the existence of a now destroyed stone circle of which three stones remained in his day, which lay about half a mile to the west of the Nine Stones, and appeared to have been of similar dimensions (PLATE I).

J. AUBREY, *Monumenta Britannica*, f. 35.

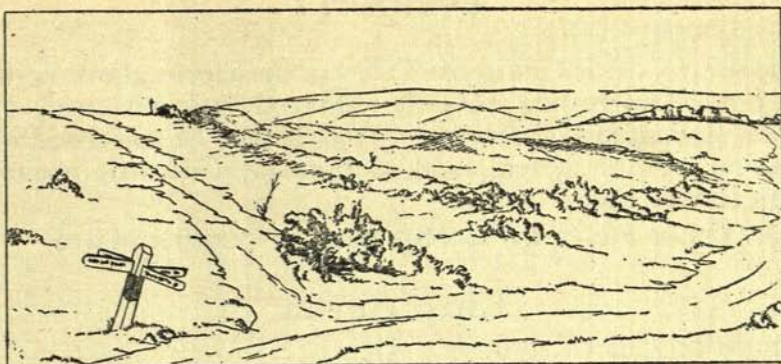
3. CIRCLE NEAR LULWORTH

Between East Lulworth and Povington a possible stone circle is recorded as having existed until the XIX cent. (Not on map, fig. 1).

C. WARNE, *Ancient Dorset*, 136.

OMITTED SITES

Two sites have been omitted which have in the past been claimed as stone circles. Of these, that above Pokeswell seems undoubtedly to be a stone ring within a barrow, of a type mentioned in the Introduction.



EARTH CIRCLE ON EGGARDON HILL

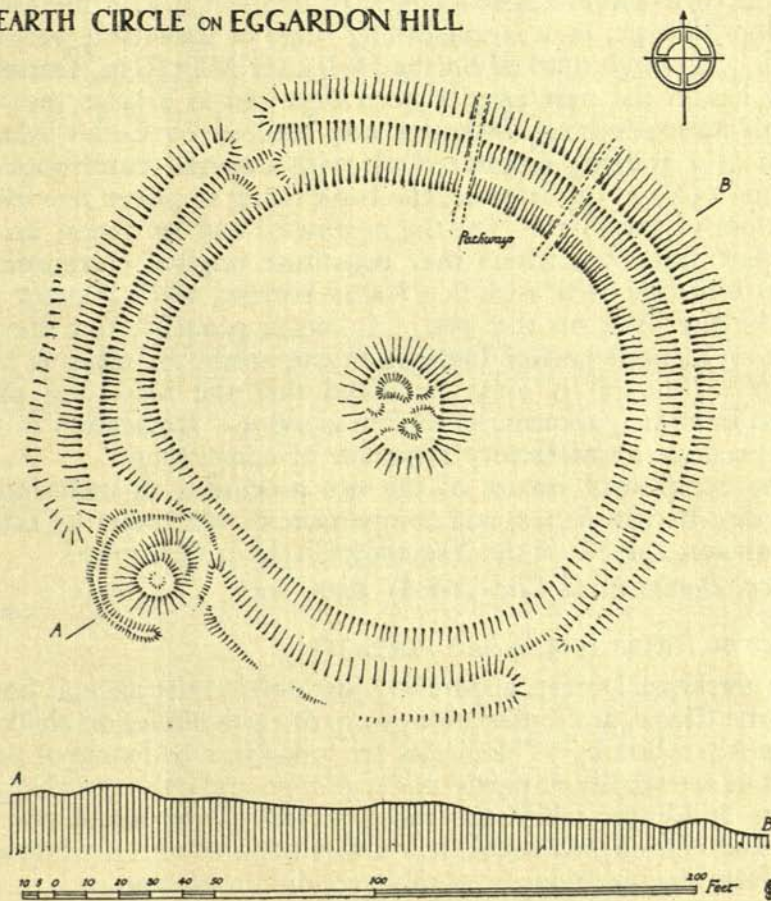


FIG. 8

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The second site is that in the Valley of the Stones above Portesham. This is a most interesting, roughly D-shaped enclosure made of small closely-set stones, and it seems likely that it is to be associated with the surrounding lynchet system, and to have been a cattle enclosure of Iron Age or later date.

Proc. Dorset Field Club (1908), XXIX, pp. lxxviii and 250.

Earth Circles

THE EGGARDON CIRCLE (FIG. 8)

This earth circle lies at a height of 800 feet just to the east of the Eggardon hill-fort, on a commanding ridge of downland west of Dorchester. Although marked on the Ordnance Map (6 in. Dorset, XXXIX NW), it has in the past usually been regarded as a large disc-barrow, although its exceptional character was realized by Colley March. It consists of a roughly circular ditch with external bank enclosing an area some 150 feet in diameter, the bank rising in places five feet above the bottom of the ditch. On the northwest and southeast are breaks in rampart and ditch which may constitute original entrances. Near the centre of this earthwork is a round barrow, while another appears to overlie the bank on the west. It seems probable that the first of these is an integral part of the monument, while the other is probably secondary, although it must be noted that the bank and ditch are flattened in its neighbourhood as if to avoid it. Excavation of this site should produce a satisfactory sequence of construction.

The exceptional nature of the site precludes its inclusion in the normal disc-barrow series, and comparison should rather be made with the Knowlton barrow or the Ysgeifiog Circle in Flintshire.

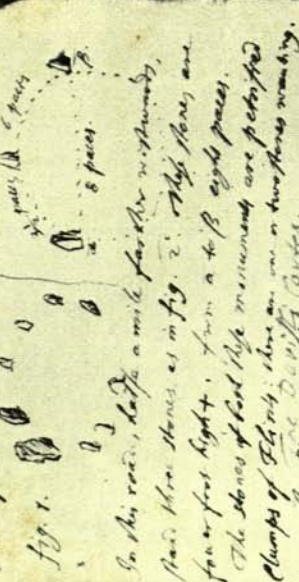
Proc. Dorset Field Club (1908), XXIX, lxxv.

KNOWLTON RINGS (FIG. 9 and PLATE II).

In northeast Dorset, apparently just outside the ancient bounds of Cranborne Chase, is a remarkable group of earth circles on chalk upland near the Allen brook. The circles are today in a bad state of preservation: four are wholly or partly under plough and one is further defaced by farm buildings, while the most perfect circle has a now-ruined church and churchyard within it. Other features of the group are only visible from the air under favourable conditions of crop.

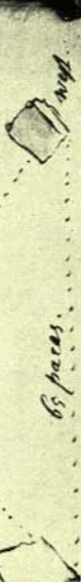
As can be seen from the plan, the group consists of three main circles, the Northern, Central and Southern; an earthwork known as

In an enclosure in the parish of Winterbourne
about four miles from Dorchester, in the County of Dorset
a little within the hedge near London road are found certain
stones, nine in number, circularly or in this form:
The highest of them is seven feet in diameter
about five feet in height, the others are broken and now are
about a yard high. The diameter of the stones
is more or less.



In this road, half a mile farther westward,
near three stones as in fig. 2. These stones are
four feet high, from a to b eight paces.
The stones of this type are now in a state of decay.
Clumps of Fuchsia; there are now in two places remaining.

In the stone-enclosure, which in 1790 was
removed from the stone field two great stones, called
the De Witt's Coyles, sixty five paces distant from
one another. The east stone is nine feet high, and is now
broken. It is a 7 ft. high. The west stone is eight feet
high & about six feet across. It is a 7 ft. high. There are
three English feet from base to a great Barrow.



The greatest segment of a circle, is the imaginary circle
which has passed the stones & is 10 paces.

At Brome near Swanton in Wiltshire in the middle
of a pasture ground called Longstone, is a great stone four
feet high (or better) standing upright, which made to be the
Remainder of this kind of temple in the ground.
It is a 7 ft. high. The ground is a great Barrow.
in a right line. The ground is a great Barrow.
in a right line.



These stones are to be better put in one place, as here.

Mr. St. John, who is now standing, seems to hint,
that all places having that name, have been just
monuments: e.g. St. John's Town, St. John's.

* One of these stones was taken down by a farmer
about year 1680, he makes a bridge of it.

PLATE II



OBLIQUE AIR-VIEW OF THE KNOWLTON CIRCLES FROM THE EAST, SHOWING THE DITCH ROUND THE
LARGE BARROW (see p. 152)

Crown copyright reserved

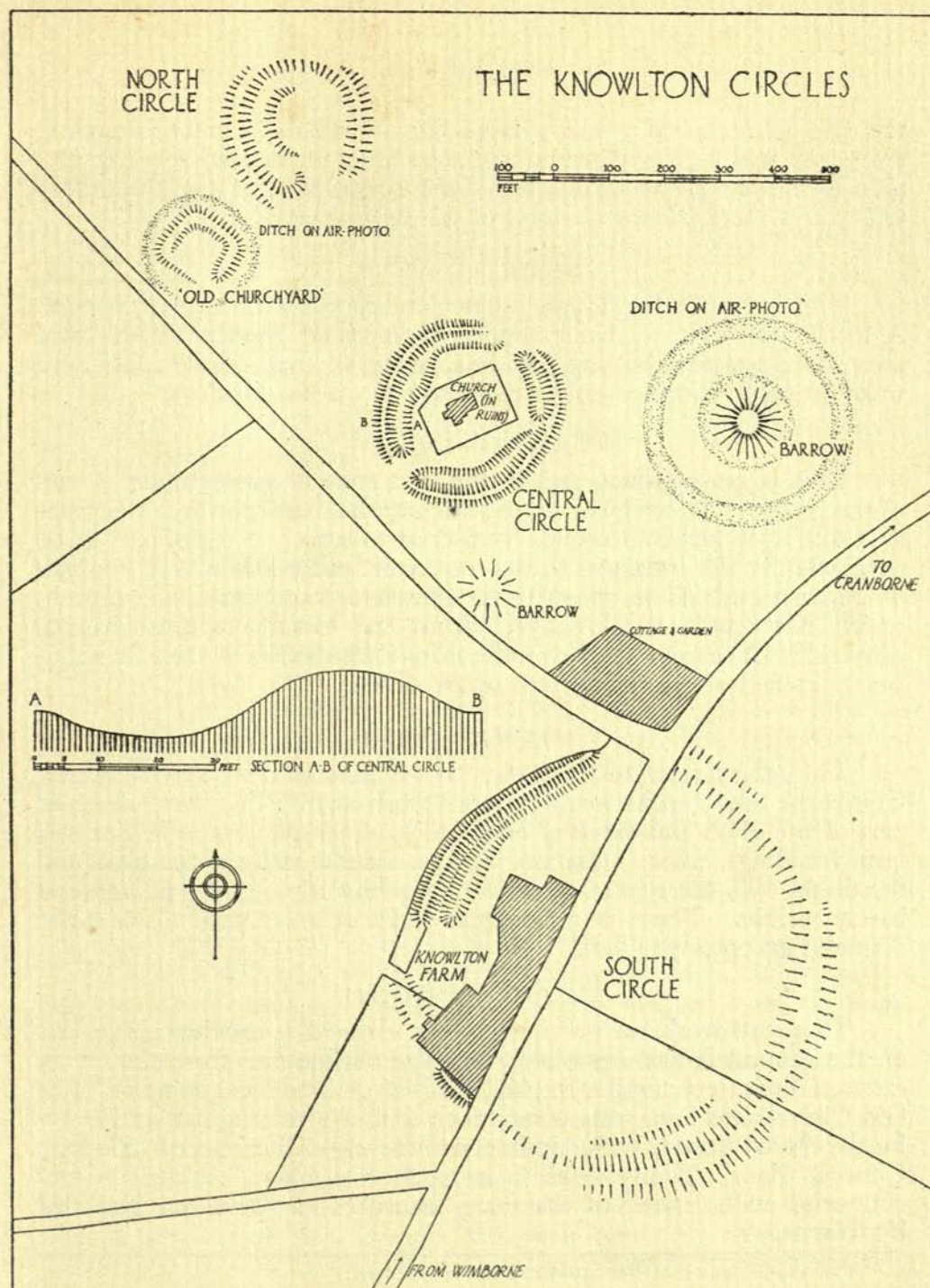


FIG. 9

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the Old Churchyard ; and a large barrow within a great encircling ditch. It will be convenient to describe them in this order. The air-view is reproduced by permission of the Controller of H.M. Stationery Office and the Director General of the Ordnance Survey.

NORTHERN CIRCLE

This is now badly ploughed down and its original features obscure. A ditch with external bank appears to have enclosed a slightly oval area some 200 feet by 225 feet from crest to crest of the bank, and today a single entrance only can be traced on the southeast.

CENTRAL CIRCLE

This is grass-grown, and in the best state of preservation of any of the group. An irregular ditch with external bank encloses a circular area some 300 feet in diameter from crest to crest ; it is difficult to be sure whether the entrances to the southwest and northeast are original. The internal area is occupied by the churchyard and the ruined church, which has architectural features from the twelfth to the sixteenth centuries. As can be seen from the section, the bank rises to a height of nearly 10 feet above the bottom of the ditch.

SOUTHERN CIRCLE

This, the largest of the series, is cut through by the Wimborne-Cranborne road and its western part is built over. The best preserved part of the ditch and external bank is in a small plantation behind the farm buildings, where a distinct berm is visible between the bank and the ditch. To the east of the road the earthwork is under plough and barely visible. There is no definite trace of an entrance, while the diameter averages 750 feet.

THE OLD CHURCHYARD

This earthwork has previously been claimed as another circle, but on the ground it has a markedly angular outline and from the air an *external* ditch (less angular in plan) is visible. The local name of 'The Old Churchyard' suggests comparison with the rectangular enclosures in the New Forest with similar names—*e.g.* Churchyard, Sloden ; Church Place, Denny Wait ; and Church Place, Ashurst¹⁰—and connexion with legend of the many churches of Knowlton recorded by Warne.

¹⁰ Heywood Sumner, *Earthworks of the New Forest*.

STONE AND EARTH CIRCLES IN DORSET

GREAT BARROW

This large tree-covered barrow, 20 feet in vertical height and 125 feet in diameter, is seen from the air to be surrounded not only by its own quarry-ditch but by a large encircling ditch 110 feet from the edge of the mound. A slight hollow marking this outer ditch, which has a diameter of 325 feet, was visible on the ground when the writers examined the site in 1936. The whole structure compares well with the Eggardon Earth Circle.

A small ploughed-down barrow is shown on the plan between the Central and Southern Circles : several others exist in the immediate vicinity of the circles today, while air-photographs hint at many others now completely ploughed away.

Map of Neolithic Wessex, no. 167, with full references.

MAUMBURY RINGS (FIG. 10)

The earthworks of Maumbury Rings lie on the level ground on the southern outskirts of the town of Dorchester, forming a conspicuous object from the Weymouth road. The subsoil is chalk and the site is now grass-grown and a public resort.

First noticed by Sir Christopher Wren on his journeys to Portland when obtaining stone for building St. Paul's, it was a little later described by William Stukeley as a Roman amphitheatre. Between 1908 and 1913 excavations were carried out on the site by Mr H. St. George Gray, and it was then found that with the exception of a Civil War gun emplacement to the southwest, the monument as it stands today is essentially a prehistoric work which was adapted by the Romans for the purposes of an amphitheatre.

No definitive publication of Mr Gray's work has yet appeared, and the following summary of the prehistoric features revealed is based on his five interim reports. The plan has been adapted from his small-scale sketch-plan.

The cuttings into the bank showed it to be of uniform construction and a single-period work, with a vertical height of 15 feet above its old turf-line. No Roman objects were found in the make-up ; antler picks and a piece of chalk carved into a cylindrical form being the only finds. It was found that the Roman adaptation of the site had involved the levelling of the internal area in such a way that no less than 11.75 feet of solid chalk and top-soil had been removed, thus effectually destroying any original features such as the sockets for possible standing

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stones or posts. Furthermore, this levelling had resulted in the internal quarry-ditch for the rampart being represented today by its lower part only. This ditch, invisible on the ground before excavation, had been dug in a most remarkable manner, consisting in effect of a number of

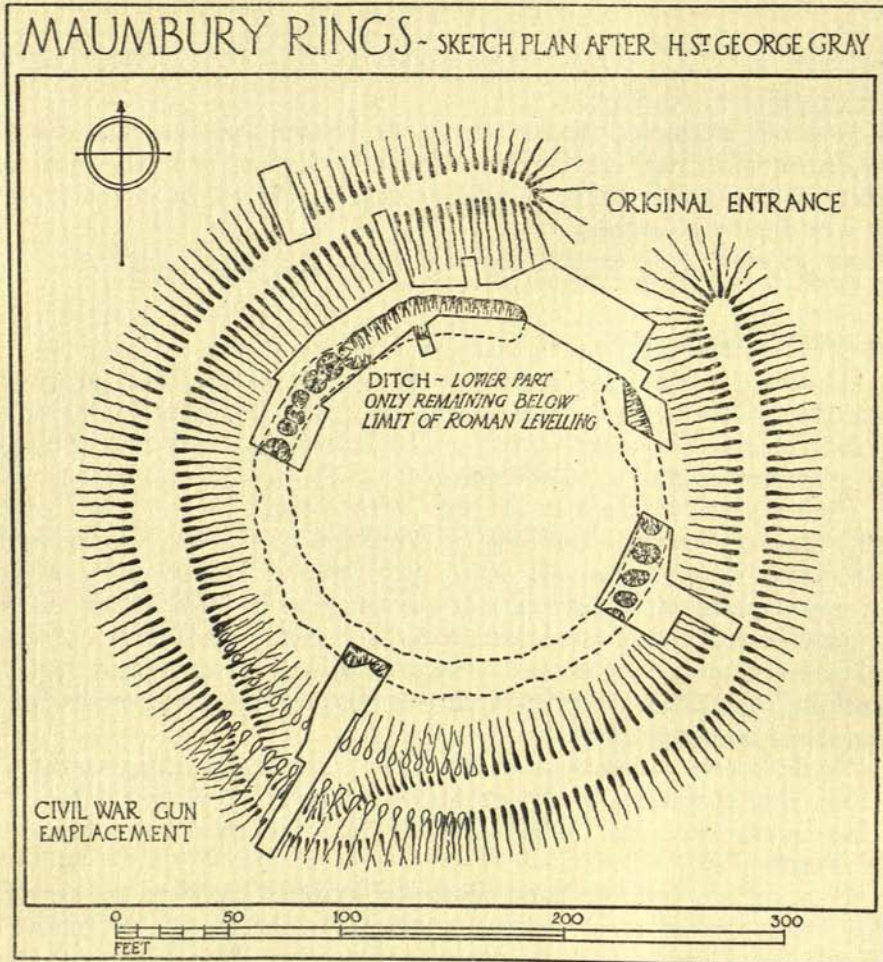


FIG. 10

very deep funnel-shaped pits arranged in a contiguous series, in such a manner that their mouths must originally (before the Roman levelling) have coalesced to form a continuous, if irregular ditch. Seven of these pits were excavated to the bottom, their average depth beneath the original

STONE AND EARTH CIRCLES IN DORSET

(pre-Roman) level being estimated at 35 feet. This extraordinary method of quarrying for rampart-material seems to be without parallel.

This ditch, and the exterior bank, were found to have enclosed a circular area 280 feet in diameter from crest to crest of the bank, with a single original entrance to the northeast. At one point it was seen that the ditch had cut through an earlier feature in the form of a circular hole 2.2 feet in diameter, which may have been a posthole, but otherwise no features of prehistoric date survived except the bank and ditch.

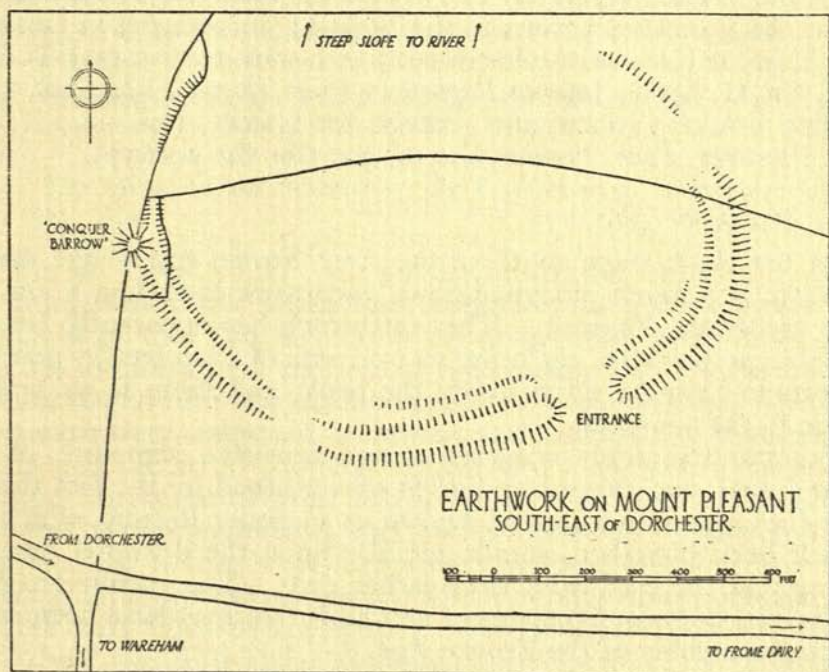


FIG. 11

It is clear that these formed a monument of the Woodhenge-Arminghall class, but it is now impossible to know whether stones or wooden posts stood within the area. A single stone of some size appears to have existed on the western side of the entrance until 1846, when it was buried below the reach of ploughing, but neither an excavation of 1879 nor those of 1908-13 revealed any signs of this.

The finds from the ditch were scanty, being mainly in the form of antler-picks, but there were also flints of characteristic Early Bronze Age facies and a sherd of cordoned pottery allied to the Groove-Ware

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series. Carved chalk objects included cups, scratched blocks and fragments of large phallic representations recalling those from Neolithic sites.

In addition to the Roman adaptation of the site, it further suffered from being used as a Parliamentary fort in the Civil War, when the internal terraces and the gun emplacement opposite the entrance were constructed.

W. STUKELEY, *Itinerarium Curiosum*, I, 163-175. The same account was printed separately in 1723 for the members of the masonic lodge at the Fountain Tavern in the Strand; reproduced in facsimile by the Quatuor Coronati Lodge in 1925 with introduction by E.H.D.

H. ST. G. GRAY, *Interim Reports in Proc. Dorset Field Club*, XXIX, 269-272; XXX, 233; XXXI, 260; XXXIV, 100; XXXV, 110.

S. PIGGOTT, *Proc. Prehist. Soc.* II, 200 (for the pottery).

MOUNT PLEASANT (FIG. 11)

On the chalk ridge south of the river Frome and to the east of Dorchester is a much ploughed-down earthwork encircling a low hill known as Mount Pleasant. This earthwork has apparently been of formidable proportions, enclosing an oval area of 1200 feet by 900 feet. It appears to have the ditch inside the bank, and there is an original entrance to the southeast.

The internal ditch suggests a non-defensive structure of the 'Henge' class, and this suggestion is strengthened by the fact that on the west a large round barrow, known as Conquer Barrow, which has not itself been ploughed, stands actually upon the denuded rampart, implying that the earthwork is of earlier date. The barrow has been dug into and no record remains of any finds that may have been made, but it is presumably of the Bronze Age.

These facts, taken in conjunction, justify the inclusion of the Mount Pleasant site as a possible Early Bronze Age monument of the type represented in Wessex by the second period at Avebury or Durrington Walls; the southeasterly entrance suggesting, for reasons given in the Introduction of this paper, the original existence of another to the northwest, thus distinguishing it from the allied single-entrance monuments of which Maumbury Rings is typical. Excavation alone can prove or disprove these very tentative suggestions.

C. WARNE, *Ancient Dorset*, 242 - 'vestiges of a large rectangular entrenchment, which I hold to have been Vespasian's Camp'. Mr Crawford first drew attention to its peculiar relation to the round barrow.

A Mesopotamian Trilogy

by M. E. L. MALLOWAN

1. LA CIVILISATION D'ASSUR ET DE BABYLONE. By Dr G. Contenau, Conservateur des Antiquités Orientales au Musée du Louvre. Paris: Payot, 1937. 30 francs.
2. FOUILLES DE TELLOH: sous la direction de H. de Genouillac. Vol. II, Époques d'Ur IIIe Dynastie et de Larsa. Paris: Paul Geuthner, 1937. Vols. I and II. 400 francs.
3. THE MUSIC OF THE SUMERIANS. By F. W. Galpin. Cambridge University Press, 1937. 18s.

THESE three works, all published in the same year, form a trilogy with the Tigris and Euphrates as a background. The Land of the Two Rivers, as historians have aptly called it, was from the beginning of the fourth millennium B.C. a focus on which man converged from the cardinal points of western Asia. The earliest settlements were composed of farmers and hunters: in the south their pottery and the art of painting suggest that they were predominantly Iranian; in the north their arts show that they were in closer touch with the Syrian hinterland and eastern Anatolia. Sometime before 3000 B.C. the invention of the wheel, of metal-working and writing produced an industrial revolution, which brought the young civilization of these early farmers to maturity. The beautiful painted pottery of Al 'Ubaid and Tall Halaf died out, partly because, as de Genouillac says, the invention of cuneiform writing made the pictorial writing of clay vases obsolete, and no doubt caused the skilful artisans of painted ceramic to apply themselves to the more paying craft of metallurgy, much as in western Europe in the first quarter of this century a horde of stable hands left the paddock for the garage and forsook the horse for the motor car.

Metallurgy brought with it a far greater efficiency in the weapons of war: the training of well equipped armies enabled the best organized cities to obtain greater stability and make enduring conquests. The growth of strong cities inevitably opened the way to Empires, and enabled the Akkadian dynasty of the 25th century B.C. to extend their

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frontiers to the Mediterranean coast, Anatolia and Iran. The greater security of trade routes facilitated the rapid spread of ideas and inventions. Further, the exploitation of metals, which led to constant improvements in furnaces, transformed mud-brick into burnt-brick and induced the full development of architecture. An unparalleled expansion in trade, bringing with it universal economic prosperity and increase in population, required larger and more permanent buildings; the adornment of temples ennobled sculpture and produced the masterpieces of Gudea in the 24th century B.C. The wheel, which induced both the mass production of pottery and a greater mobility of transport, extended the limits of trade. Finally, the development of writing laid the foundations of science, and in Ashurbanipal's library, the equivalent of that of the Ptolemies at Alexandria, we have in the 7th century B.C. the accumulated lore of western Asia. History, astrology, astronomy, chemistry, medicine, mathematics, geography and language, as we have them in the twenty five thousand tablets from the Kuyunjik collection of Nineveh, the result of nearly 3000 years of careful scholarship, laid the foundations without which Greek science could never have developed as it did and without which mankind today would be at a very different stage in human progress.

Who were the peoples responsible for this dawn of civilization? Dr Contenau has given us a sound and clear resumé of the question. If we turn to the monuments we find that from the very beginning there were two distinct racial types in Mesopotamia. On the one hand there were the Sumerians with their round heads and low receding foreheads, brachy- or mesocephalic in type with a prominent nose rather like an eagle's beak; on the archaic statues (early 3rd millennium B.C.), the hair was worn long and divided into two tresses falling on to the front of the shoulders and there was a long, square and carefully trimmed beard. Later on this type was often represented as clean shaven. On the other hand there were the Semitic Akkadians, less frequently represented. The profile was less accentuated, the nose straight or slightly aquiline, and as far as one can judge the head was no longer globular—but the shape was usually concealed by hair. Thirdly, Dr Contenau believes that we can trace an autochthonous Asianic type, linguistically allied to the Sumerians, certainly not Semites and probably not Indo-Europeans. The stone heads recently discovered at Brak (before 3000 B.C.) may perhaps belong to this third category, for they are neither Sumerian nor Akkadian in appearance.

When we come down to the first millennium B.C., nearly 1500

A MESOPOTAMIAN TRILOGY

years later, we find the Assyrian type which Dr Contenau describes as the 'Israelite classique', in many respects similar to the Armenian whose territory the Assyrians in part occupied. An archaic statue found at Bismaya is suggested as the forerunner of this type.

But the difficulty is that although the linguistic evidence accords with that of the monuments in differentiating three groups of peoples, we have a discrepancy between this testimony and that of the craniological evidence, which shows that in the third millennium B.C. the brachycephalic heads of the monuments were a great rarity, the skulls from the Royal Cemetery of Ur for example being predominantly dolichocephalic or modern Arab in type. It is impossible therefore satisfactorily to account for these discrepancies, but it leads to the conclusion, I think, that we must be wary of attempting to argue racial characteristics from the archaic monuments of the early third millennium B.C. We have to remember that portrait-sculpture developed relatively late in Sumer, for we can point to very few heads that strike us as individual rather than types, till we come to the magnificent bronze head of the 25th century B.C. discovered at Nineveh, and presumed by some to be a portrait of Sargon of Akkad himself¹ (PLATE I). At most we can say that the Sumerians represented themselves as a very different type from the Semitic Akkadians and the later Assyrians, but their sculpture was bound by severe canons, and the result is that we have an impression rather than a photographic likeness of the type.

When we turn to the archaeological evidence it is no less difficult to determine at what precise period the Sumerians themselves entered the field. In the early stages Dr Contenau diagnoses four precise lines of demarcation in the successive phases of civilization between Tigris and Euphrates, as follows:

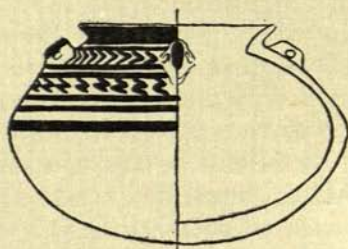
(I) THE AL 'UBAID PERIOD. Characterized by a hand-made painted pottery with dark on light designs, mostly geometric. This pottery persisted in degenerate form in regions where the later Uruk and Jamdat Nasr ware did not appear, *e.g.* Susa II and Giyan IV.

Dr Contenau distinguishes two manifestations of the Al 'Ubaid phase: (a) the Al 'Ubaid pottery which is predominant in the southern alluvial country; (b) the Tall-Halaf-Arpachiyah pottery which is predominant in Assyria (PLATE II A, B). But I think that the differences between these two classes of pottery have now proved to be fundamental and that we ought no longer to group them under a single heading.

¹ M. E. L. Mallowan, 'The Bronze Head of the Akkadian Period from Nineveh', *Iraq*, 1936, III, part 1. (See note on p. 170 of this article).

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There is indeed some evidence to suggest that certain wares of Al 'Ubaid type may have owed their development to a contact with a late phase of T. Halaf-Arpachiyah pottery—discoveries at Tepe Gawra and in the Balikh valley lend colour to this view. Moreover, it is becoming increasingly apparent that between Tigris and Euphrates T. Halaf-Arpachiyah ware must be ascribed to an older civilization, the marks of which are to be found as far afield as Ras Shamra on the Mediterranean coast, Cilicia in southwest Asia Minor and in Van. What is more, we now know that behind T. Halaf and the early chalcolithic painted pottery of Nineveh there is a yet older monochrome ceramic which must belong to an earlier phase still. In short the prehistoric Assyrian or rather Subaraean ware of T. Halaf must be



AL 'UBAID WARE FROM ARPACHIAH

sharply distinguished from its Iranian cousin of Al 'Ubaid and we must recognize that it began earlier in time.

(2) THE URUK PERIOD. Characterized by a metalliform red slipware, gradual abandonment of painted pottery and the beginnings of wheel-made pottery. The period is notable for the construction of monumental stone and mud brick temples, the use of mosaic columns and the beginnings of sculpture. The older forms of stamp seal gradually give way to the cylinder seal. Writing is invented, in the form of a pictographic script developing into a cuneiform. The period probably covered several centuries and provides us with the first synchronisms with predynastic Egypt (PLATE II D).

(3) THE JAMDAT NASR PERIOD. Characterized by a wheel-made polychrome pottery, often trichrome, which in Sumer is a last attempt at reviving a painted pottery. This ceramic is less common and less dispersed than that of previous periods. Outside Sumer it occurs in Iran at Susa and Tepe Moussian; its latest development is the scarlet ware of the Diyala region in upper Mesopotamia. Otherwise,

A MESOPOTAMIAN TRILOGY

the Jamdat Nasr is merely a development of the Uruk phase ; writing develops into a fully fledged cuneiform and becomes common together with an increasing use of metal. There are synchronisms with Egypt at the beginning of the Thinite period (PLATE II C).

(4) THE EARLY DYNASTIC PERIOD. Characterized by a climax of development in architecture, sculpture and metallurgy and by the discovery of monuments which may now be attributed to historic names mentioned in the early king-lists. This period begins after 2900 B.C.

The problem which we must now face is at which of these periods the Sumerians made their entry. Dr Frankfort, tracing back the origins of architecture and the handicrafts, and finding a common ancestry in the Al 'Ubaid period, insists that the Sumerians were there from the beginning,² whereas Dr Jordan believes them to be the originators of the monumental civilization of Uruk. Others would place their arrival as late as the Early Dynastic period, but the epigraphic and monumental evidence from Uruk puts this theory out of court. Dr Contenau argues against Dr Frankfort that, inasmuch as the Sumerians in their full development exterminated painted pottery, it is difficult to attribute to them the civilization of Al 'Ubaid, a civilization which has painted pottery as its hall-mark. Dr Contenau therefore agrees with Dr Jordan that the Sumerians first entered Mesopotamia in the Uruk period when the constitution of Sumerian art was first clearly defined and writing made its first appearance.

As the Al 'Ubaid peoples must remain inarticulate owing to the absence of writing, the matter is one that admits of speculation rather than proof. It seems to me possible that the truth lies between the two hypotheses—that the Sumerians were in fact present from the very beginning in Mesopotamia, together with other ethnic groups, and that they arrived in waves. As others have argued, they were a vigorous people, probably of mountain origin, and by the Uruk period they felt themselves sufficiently strong to sweep away the older forms of civilization which their forefathers had found in the land. The sudden outburst of invention in the Uruk period may be the product of a young people with an inherent capacity for development, hitherto prevented from maturing owing to the lack of a suitable environment. Once settled in the plains they soon obtained the upper hand over the autochthonous elements and developed a new civilization still linked

² H. Frankfort, *Archaeology and the Sumerian Problem*. The Oriental Institute of the University of Chicago Studies in Ancient Oriental Civilization, no. 4.

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to the old by geographical and hereditary ties, yet firmly stamped with the new die.

But in point of fact this problem of origins is shrouded in the mists of obscurity. For in general man evolves by leaps and bounds, and invention is so sudden that we can no longer recognize the primitive stage from which it emerged. Just as, for example, we shall never discover the origin of the brilliant painted wares of T. Halaf, for the very good reason that a technical trick of washing and firing a specially selected clay enabled the potter suddenly to produce a ware which had the consistency of a China clay, with no more resemblance to the earlier wares than a ship has to a tree, so we shall probably never recognize the forefathers of the Sumerians even if eventually we discover them in some far distant Oriental mountain home.

As to the origin of the Semitic-speaking peoples there is an even greater obscurity. Up to date there is no evidence whatever to show that they came from Arabia or that there is any analogy between the spread of prehistoric Semitism and that of Islam. On the contrary, Syria would seem to be one of the earliest cradles. Incidentally we may note that Syria may be the background from which the Semitic element in the ancient Egyptian language was derived, and that it must have been Syria that served as a base for transmitting the innovations of Uruk and Jamdat Nasr to predynastic and early dynastic Egypt.

But it is a mistake to suppose that we can neatly separate the Sumerian from the Semitic elements in the beginnings of Mesopotamian civilization. As far as we can go back Sumerian and Semitic characteristics and inventions were inextricably intermingled; and from before the third millennium B.C. Sumerians and Semites must have been living together in the same cities in a national unity, much as today the Kurds are completely identified with Islam and often intermarried with the Semitic-speaking Arab.

Yet when the Semitic-speaking peoples first emerge as decisive masters of Mesopotamia under the First Dynasty of Babylon in the 19th century B.C., we may perhaps more easily contrast as specifically Sumerian certain elements which had disappeared after the age of Gudea. In particular, it seems likely that the brilliant statuary of that age, notable for the many representations of the king carved in dolerite and other hard stones, represents the fullest development of Sumerian stone carving.³ These rigid statues in which the human form remains as it

³ cf. The Patesi from Lagash in the British Museum, BM.122910 (PLATE III) and the Statues of Gudea in the Louvre.

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were firmly welded to the block which gave it birth, stand as the emblems of the stolid Sumerian genius. Here we have the impression of a virile and disciplined power bound by an age-long canon of tradition, nevertheless inventive and alert. No statuary of the kind ever again happened in Mesopotamia (PLATE III).

With the Sumerian feeling of the Gudea statues I would contrast the supple and lithe brilliance of the Akkadian statuary as seen in the Stele of the Vultures, wherein the Semitic genius seeks release from the more cramped and stolid forms of Sumer. Likewise in the Akkadian period on the cylinder seals we see an art in which there is less overcrowding. The field is broadened and emptied of unnecessary encumbrances; we may compare the new art to a man who intends to travel light, and, having shrugged his shoulders and got rid of old burdens starts on a new road, all the fitter for abandoning the heavy trappings to which he has been accustomed.

After 2000 B.C. the Semitic contribution was in fact a sifting and repairing of the encumbrances of the old order. In religion the pantheon was reduced and simplified, a single god, Marduk, imposed his authority over all the lesser deities of Babylonia—it was the first direct impulse towards monotheism. Likewise religious scenes and emblems were considerably changed—the Semitic influence of Syria is seen in the representations of the god Martu and his club. In law the code of Hammurabi modified and improved the older and scattered Sumerian codes; and Dr Contenau notes that often, as in agrarian legislation, the Semitic law had a greater regard for equity, whereas on the contrary it was always inexorable and had less regard for clemency than the older Sumerian law.

An interesting selection of small objects belonging mostly to the last quarter of the third millennium B.C., the period at which the Sumerians were at their swan song before the oncoming of the vigorous new dynasty of Semitic Babylon, may be seen in the Abbé de Genouillac's second volume on Telloh, the ancient Lagash, a term which properly speaking refers to the country, whereas the town was called Girsu by the Sumerians.

De Genouillac has with great labour recorded his share in the excavations and illustrated them by a large number of plates; they recall a work, which beginning with de Sarzec in 1877, has been a rich field for French archaeological research. But one is bound to make a serious criticism of this last contribution because the context of the

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discoveries has hardly ever been described, an omission which robs the work of the evidential value which it ought to have. De Genouillac's scholarship has enabled him to attribute the greater part of the finds to their proper periods and he has much interesting and illuminating comment. But I would suggest (a) a further short appendix in which the actual find-place of the objects be described and (b) that wherever possible the material be related to comparable finds from other sites. A short account of this kind would tremendously enhance the value of the work and incidentally would remove the misleading impression produced by pictures of objects which do not belong to this period at all.

Lagash, which lay about 50 miles northeast of Ur, was reckoned to be one of the principal cities of Mesopotamia, and enjoyed two special periods of prosperity during the first and last quarters of the third millennium B.C. The town was enclosed by a sacred wall and the different quarters linked by well-paved streets; many of the buildings were situated on the quays of canals. Between 3000 and 2000 B.C. we have records of at least 80 temples in the city and not less than 15 gods. Gudea, in the 24th century B.C., boasted of being shepherd of a flock consisting of 60 *sar* of men, or 216,000 souls; no doubt this figure does not underestimate the number of his dependents. Gudea perpetuated the worship of Nin-Girsu daughter of Anu, patron saint of the city. One of the best preserved temples was that of Nanše, dispenser of laws, for whom four months in the Sumerian calendar were set aside. De Genouillac suggests that the many infant pot-burials under the floor of this temple were holocausts of new born children. If that is true it is surprising that we have no record of such a practice in contemporary cuneiform texts. The same problem also arises on other Mesopotamian sites such as Ur, where the chapels attached to private houses of the 19th century B.C. frequently contained pot-burials with the bodies of infants.⁴ It seems to me that the theory of holocausts is not proven and that these pot-burials with many bodies are rather to be accounted for by a high infant mortality which gave rise to the adoption of child cemeteries, for in fact infants are rarely if ever found in the family vaults which lie beneath the floor of every house at that period. So

⁴ *Antiquaries Journal*, VII, no. 4. Note the suggestion by Sir Leonard Woolley who denies the theory of infanticide, and that the infant burials in the Ur houses were consecrated to a particular patroness of little children. One of the Ur houses contained the bodies of 32 infants.

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far as we know infant sacrifice is a Semitic practice attested at Gezer and in the Punic city of Carthage,⁵ but not in Mesopotamia.

Another important architectural find was a princely hypogeum containing four tombs, close to the temple of Nanše. De Genouillac began the excavation of this very important building and the work was brought to its full fruition by M. A. Parrot, who discovered in two of the vaults some magnificent material consisting of seals and inscriptions of Ur Ningirsu and Ug-me, son and grandson of Gudea. This mausoleum of the Patesis must be compared with the Royal tombs of Shulgi and Bur Sin discovered by Sir Leonard Woolley at Ur.⁶

Among the many interesting objects found by de Genouillac we may call attention to the terracotta head shown on PLATE IV B, briefly referred to on page 66 of the text. The interest of this figure is that the face and mouth and more especially the style of hair dressing bear a close resemblance to the celebrated bronze head of Sargon (?) from Nineveh, and likewise therefore to the gold wig of Mes-Kalam-Dug. We must presume that the Telloh head is to be dated not earlier than Gudea: the fact that it has horns suggests that it represents a god. Its discovery strengthens our conviction that the Ninevite head is not earlier in date than the later half of the third millennium B.C. If the author could give us the context of the head from Telloh the information might be of considerable interest.

The frequency of terracotta figurines bearing musical instruments brings us to the subject of Sumerian music, which has been treated for the first time on an adequate scale in a remarkable work by Canon Galpin. This book must surely call for attention on the part of those who profess to be authorities on the history of music,⁷ for the Sumerians are shown to have had a series of more than a dozen musical instruments, and Canon Galpin believes that there is some ground for thinking that they had arrived at a system of musical notation, though certain cuneiform scholars, among them Prof. Landsberger, do not agree with this theory.

The earliest specimen of a musical instrument known to Mesopotamia was published after Canon Galpin wrote this book. It is a bone

⁵ G. Contenau, *La Civilisation Phénicienne*. Payot, 1926. pp. 137-140, 'Sacrifices humains'.

⁶ *Antiquaries Journal*, xi, no. 4, and for Parrot's description of the mausoleum at Telloh, cf. *RA.* xxix, p. 45 ff.

⁷ C. V. Stanford and C. Forsyth, *A History of Music*. Macmillan, 1937. On p. 19 Mr Forsyth describes the interest of Babylonian music as merely antiquarian.

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flute of which six stops are still extant, belonging to the end of the Al 'Ubaid period ; the next earliest instrument is a bone pipe with two stops, divided into two tubular pairs, decorated on the back with incised ornament, apparently about 10½ cms. long : both instruments were discovered by Dr E. A. Speiser at Tepe Gawra,⁸ a site not far from Nineveh in Assyria. The pipe was discovered in stratum XI-A and the flute in stratum XII ; both instruments must therefore be dated several centuries before 3000 B.C.

Our records of Sumerian music are primarily liturgical, for music was a necessary accompaniment to most of the Temple services and even the oracle was given to the strains of a cross-strung harp. In the temples the harp was heard by day and by night. Music at funerals would seem to anticipate the modern funeral march, but music was not used only in lamentation. Horn-blowing took place in the forecourt of the temple, filling it with joy ; on a Babylonian plaque there appears to be a representation of a boxing match in which the pugilists are spurred on by the sound of kettledrum and cymbals. A curious predilection of the Sumerians for representing animals as players in an orchestra must refer either to a fable, or it may be explained by supposing that animal spirits were assuaged by music. A catalogue of some musical library made in the 9th or 8th century B.C. comprises ' liturgies, royal psalms, festal songs and hymns of lamentation : there are poems of victory and heroism, folk-songs for craftsmen and shepherds, musical recitations and a long list of love-songs for both sexes '. It appears that women of various Akkadian towns were in the third millennium B.C. called upon to mourn their fate under the Gutian oppressors in an antiphonal lament. King Gudea of Lagash speaks of the appointment of a chief musician for his Lagash temple, no doubt a post of considerable importance.

The variety of instruments quoted by Canon Galpin to correspond with the cuneiform names, or as depicted on monuments, plaques and cylinder seals, includes drums, the sistrum, rattle and bell, flutes, whistles, reed pipes, trumpets, horns, the bow-shaped harp, the lyre, the two-stringed lute and the psaltery.

It is probable that many of the tubular vases commonly described as offering-tables are really drums. Many of these were discovered in the Royal Cemetery of Ur : a terracotta drum given me by my friend Prof. Schaeffer and bought by him in the bazaar at Aleppo is an exact

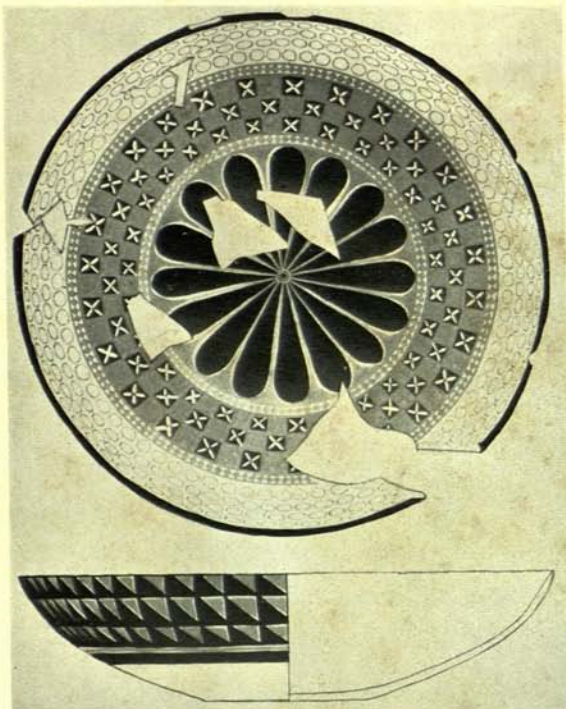
⁸ BASOR, no. 64, Dec. 1936, fig. 5, and no. 65, Feb. 1937, p.8.

PLATE I

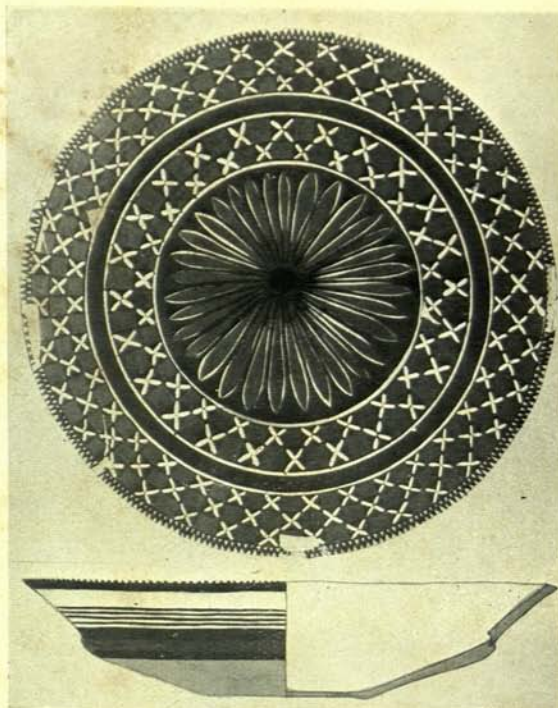


BRONZE HEAD OF SARGON OF AKKAD (?) FROM NINEVEH, 25TH CENT. B.C. (see p. 161)

By courtesy of the British School of Archaeology in Iraq



A



B

A-B. TALL HALAF WARE, FROM ARPACHIYAH (see p. 161)



C

C. TRICHROME VASE OF THE JAMDAT NASR PERIOD
(see p. 162)



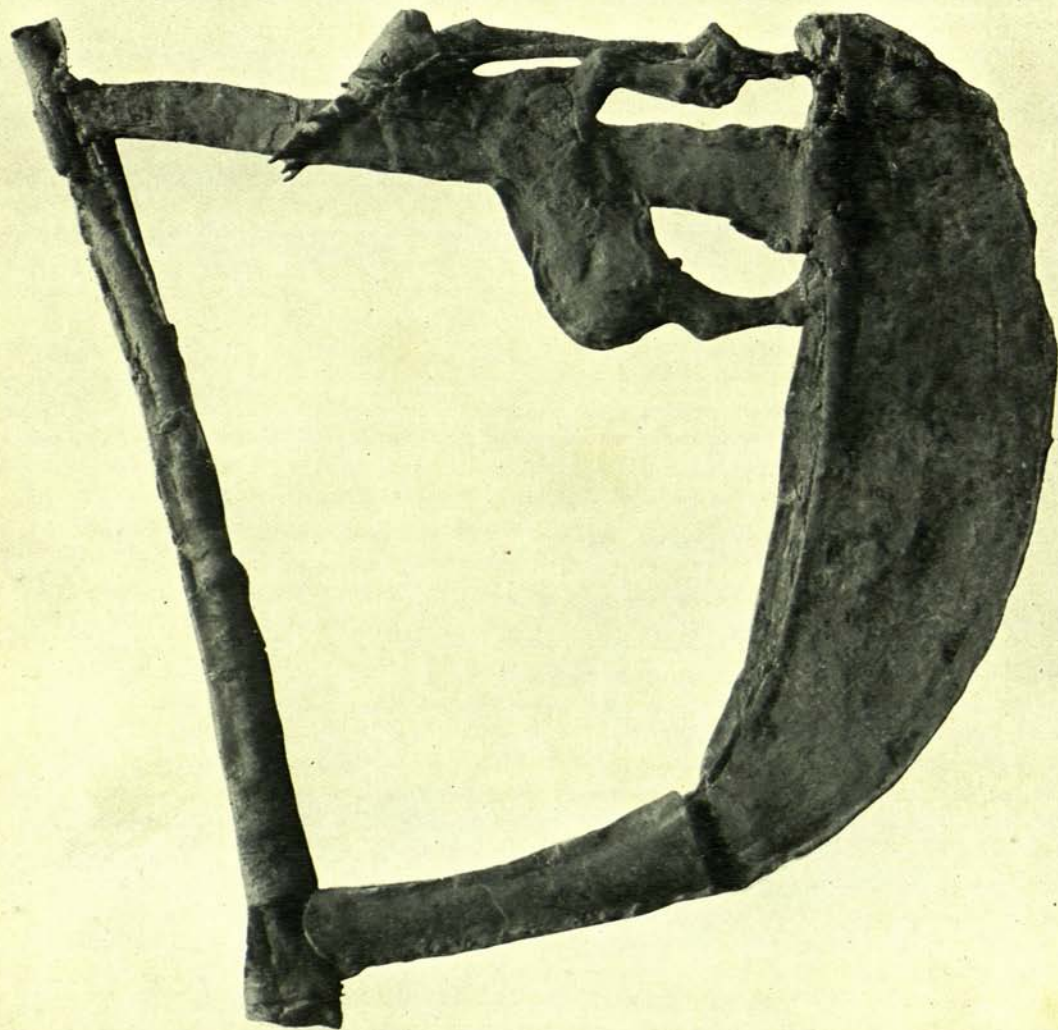
D

D. RED SLIP WARE VASE OF THE URUK PERIOD
(see p. 162)

C-D. By courtesy of the Field Museum of Natural History, Chicago



DOLERITE PATESI FROM LAGASH (see p. 164)
By courtesy of the Trustees of the British Museum



A

1. BOAT-SHAPED SILVER LYRE WITH ANTLERED STAG, FROM THE ROYAL CEMETERY OF UR, c. 2700 B.C. (see p. 169)



B

2. TERRACOTTA HEAD FROM TELLOH,
ANCIENT LAGASH (see p. 167)
By courtesy of H. de Genouillac and the
Musée du Louvre

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modern counterpart of the ancient Sumerian drum. A tablet of the Seleukid period, c.300 B.C., from Uruk, gives instructions for fastening a skin head to a bronze kettledrum in which twelve bronze images of the gods were placed.

The most elaborate instruments were the harps and lyres, the institution of the harp being ascribed to the god Enlil. A psalm in its honour says that 'its head was made of lapis lazuli and its voice, with the deep tones of its strings, sounded like that of a horned bull'. 'Its sound chest (lit. fulness) was in width like a well conditioned farmer; to it hymns of fate were recited; it glittered as the stars; it was holy; by day in the temple it uttered speech, by night it poured forth song'. The context of this poem agrees very well with the extant specimen of a large bow-shaped harp of 2700 B.C., decorated with a gold bull's head from the Royal Cemetery of Ur.

Most interesting of all are the lyres from the Ur cemetery, often highly ornamented and so well constructed that they presuppose a long period of development which must surely go back to the beginning of the third millennium B.C. (PLATE IV A).

One of the most fascinating enquiries in the book stresses the possible relations of Sumerian to ancient Chinese music. For according to a Chinese archaeologist of the 16th century A.D., one of the early Chinese emperors sent his Master of Music westward over the K'unlun Mountains to study the ordering of Western music. This journey, which dates to the period of Chinese mythical history, relates to a pre-historic era as far as the Chinese were concerned; but it reflects a very interesting tradition which links the origins of Chinese music with that of western Asia, and suggests that the Sumerian *Imin-e* or the seven-note corresponds to an original heptatonic or seven-note Chinese scale. The Chinese antiquary referred to above had seen a vertical flute in bronze dating from before the Chou dynasty (c. 1122 B.C.), producing a heptatonic scale, and suggesting a kinship with the Sumerian vertical flute.

In a stimulating chapter on the Racial Element in Music, Canon Galpin claims that 'the lyre in its most primitive form is distinctly a Semitic instrument and was so recognized by the Egyptians themselves'. In contradistinction to the bow-shaped harp which is related to instruments east and northeast of Sumer and probably Sumerian in origin, the lyre 'is a connecting link with districts westward of that country rather than with those on the east, where the lyre type is unknown'. An important appendix on the development and distribution of the bow-shaped harp suggests on the one hand early Sumerian connexions with

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India and Tibet, and on the other hand would account for its presence in Africa as due to Sumerian influence. The presence of the bow-shaped harp in Uganda is noted, together with the late survival of a remarkable burial-cult involving a holocaust of human victims and reminiscent of the sacrifices in the Royal Cemetery of Ur.

The distribution of musical instruments is therefore seen to have an important bearing on those Mesopotamian origins which we discussed at the beginning of our article. I think it is of cardinal importance to stress the bearing which music has on the possibility of an early connexion between China and western Asia, a connexion which is denied by certain scholars on archaeological grounds at so early a period.⁹ However that may be, I am convinced that eventually we shall find that from early chalcolithic times onwards there were certain contacts between China and western Asia, a conclusion which I would first argue from the family resemblance between the earliest painted chalcolithic pottery of Kansu and other regions, and that of western Asia.

At the outset of this article I referred to the three works under discussion as a trilogy. Taken as such they emphasize the interdependence of three forms of research which are essential to an understanding of ancient things. Excavation brings forth the material and presents it in a special context; history combines the results of excavation in a co-ordinate whole; special research throws a spot-light on a particular activity which reveals an intimate glimpse of man concentrating his activities in a special channel. After the recent spate of excavation in western Asia we may confidently expect year by year trilogies of this kind, which must shed a new light on the Ancient East and bridge the gaps between East and West—gaps which are today being filled with an astonishing rapidity.

NOTE. The bronze head discovered at Nineveh (see p. 161) illustrated on PLATE I was discovered by Dr R. Campbell Thompson at Quyunjik in 1931 and first published by him in A.A.A. XIX. I am indebted to him for his courtesy in allowing me to reproduce it here.

⁹ C. G. Seligman and H. C. Beck, 'Far Eastern Glass: some Western Origins', pp. 31. Bulletin of the Museum of Far Eastern Antiquities, Stockholm, 1938.

Archaeology in Palestine

by KATHLEEN M. KENYON

1. THE STONE AGE OF MOUNT CARMEL ; EXCAVATIONS AT THE WADY-EL-MUGHARA. Vol. 1. By D. A. E. Garrod and D. M. A. Bate. Clarendon Press, 1937. pp. 240, 55 plates, 8 text figures. 42s.
2. LACHISH I : THE LACHISH LETTERS. By Harry Torczyner, Lankester Harding, Alkin Lewis and J. L. Starkey. Published for the Trustees of the late Sir Henry Wellcome by the Oxford University Press, 1938. pp. 221 (including plates, not numbered separately). 25s.
3. THE QUARTERLY OF THE DEPARTMENT OF ANTIQUITIES IN PALESTINE ; VOL. VII, SELA-PETRA, THE ROCK, OF EDOM AND NABATENE. By G. and A. Horsfield. Published for the Government of Palestine by the Oxford University Press, 1938. pp. 42, 74 plates, 10 text-figures. 5s.
4. THE ANNUAL OF THE AMERICAN SCHOOLS OF ORIENTAL RESEARCH : VOL. XV, EXCAVATIONS IN EASTERN PALESTINE II. By Nelson Glueck. pp. 161, 38 plates, 45 text-figures.
5. PRELIMINARY REPORT OF THE UNIVERSITY OF MICHIGAN EXCAVATIONS AT SEPPHORIS, PALESTINE, IN 1931. By Leroy Waterman. University of Michigan Press, 1937. 2 dollars.

THE group of books here under review is so varied in subject, that a connected discussion is difficult. Apart from the importance of the subjects, however, there is a general interest and connexion in the illustrations they give of the comparatively new approach now being made to Palestinian Archaeology. The inevitably preponderant importance to most people of the association of Palestine with Biblical history has had two effects on the study of its archaeology. In the first place, periods outside that of the Bible have not received much attention, and in the second, it has rather often been the case that distinguished Biblical scholars have undertaken excavations without the supplementary training in field-work which modern archaeological technique now requires. Among the books under review are examples of how this tendency has in recent years been corrected. We see Palestine taking its place as an important connecting link between the great lands of the Near East, and we see how modern methods are now applied to all aspects of field archaeology. There is also unfortunately one example of how modern excavations should not be done.

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Chronologically earliest, and possibly also the most important, since touching less-known ground, is the first volume of *The Stone Age of Mount Carmel*. Miss Garrod and Miss Bate are to be heartily congratulated on producing a first-class report on a subject of first-rate importance, and that too at a very reasonable period after the completion of the excavations. It is a striking illustration of the backwardness of Palestine in all but Biblical archaeology that in the Wady-el-Mughara, the Stone Age industries of Palestine have for the first time been found in sequence and not in isolated deposits. Owing to the fortunate fact that there is a secure overlap between two adjacent caves, the Mugharet-el-Wad and the Mugharet-et-Tabun, the Stone Age chronology of the region from the Tayacian, through Acheulean, Levalloiso-Mousterian, Lower and Upper Aurignacian, Atelian to Natufian (the Palestinian Mesolithic), is securely established. As Miss Garrod points out, at El-Khiam, Tahunian immediately follows the latest Natufian, and at Jericho Tahunian is the industry both of the lowest non-pottery bearing levels and of the pottery bearing Neolithic levels which overlie them. It is no small triumph for a country which entered so late upon the prehistoric field to have produced so complete a framework, into which the isolated finds can now easily be fitted. But as Miss Garrod and Miss Bate point out, there is one thing still lacking, the secure fitting of the sequence of industries into the geological and climatological setting. Miss Bate's work on the animal bones of the different layers has established a number of climatic changes, but so far it is not very easy to fit them in with the evidence of the pluvial periods in the Jordan Valley. Work is clearly needed on the beaches of Palestine, by which the geochronological work done by Dr Zeuner and others, which has linked up so much of the Stone Age deposits from North Europe down to the Mediterranean coasts, can be carried round its shores to the Near East.

Many other points of interest are raised, such as the fact that the human connexions of Palestine were undoubtedly with Europe, and very little with Africa, while the animal remains show that the country served equally definitely, as would geographically appear probable, as a bridge by which North Africa received much of its Pleistocene fauna. Further, there is a great change (due to climatic reasons) in fauna to modern conditions, that is not reflected in the human industry, which merely evolves from Lower to Upper Levalloiso-Mousterian. The divergencies between the human and animal histories are rather remarkable. Of further outstanding interest is the cemetery of twelve

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persons of Levallois-Mousterian type outside the Mugharet-es-Skhul. But in a review it is not possible to deal with all this.

It is not at all easy to criticize a work which is so thorough in presentation of evidence, illustration, analysis and comparative work. Both industries and fauna are recorded and illustrated layer by layer and the correlations of both with other Palestinian sites as well as European and African ones are fully worked out. Miss Bate's time-chart of the distribution of species is of particular interest. The drawings both of implements and bones are excellent. The finish of some of the photography on the site could, however, be improved on in matters such as cleaning, removing extraneous objects and positions of scale. A small criticism of the drawing of the skeletons is that their appearance is spoilt by such very heavy and ugly north-signs. But these are small matters in such a first-class work.

The interest and importance of *Lachish I* is of a very different sort from that of the work just considered, but an equally great one. This beautifully produced volume will serve as a permanent memorial to Mr J. L. Starkey, the finder of the Lachish letters with which it is concerned, whose tragic death just preceded its appearance. All archaeologists must deeply regret that a piece of particularly wanton terrorism has deprived them of such an able colleague, and prevented Mr Starkey from carrying to completion the work he so brilliantly began.

The excavations of Tell Duweir have been going on for five years, and the interim reports have indicated the importance of the finds. This first volume of the definitive report is concerned purely with the remarkable collection of eighteen ostraca found in a chamber of the gateway, and it is mainly the work of Professor Harry Torczyner of the Hebrew University of Jerusalem. It will come as a shock to the non-specialist to realize that this is the first collection of original literary documents of the pre-Exodic period which has ever been found. The extensive collection of ostraca from Samaria are concerned with business records, and besides that there is only the Siloam inscription and names on seals and weights. The ostraca consist of a number of letters written on potsherds, and their importance on epigraphic, lexicographic and historical grounds cannot be over-estimated. The presentation of the evidence is admirable. The ostraca are illustrated by photographs and hand copies, and these are accompanied by transliterations, translations, free translations and discussions of the language and meaning.

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The interpretation of the meaning of the letters is naturally the aspect which will interest the archaeologist and historian most. As with all ancient records of this type, considerable ingenuity is necessary in this work, and Professor Torczyner's arguments will not always convince the reader. The ostraca were found in a heavy burnt layer of a guard-room in the gate, which had been rebuilt over another burnt layer. The existence of these two burnt layers, found over a large area of the city, and from the evidence of the contents only separated by quite a short period, Mr Starkey explained by postulating an otherwise unknown destruction by the Babylonians of the city *c.* 597, in addition to the one known from the Biblical narrative in 588. The letters would therefore belong to the period between the two. They do not appear to be archives in the usual sense, for they are all letters from one man, Hosha'yahu, to his superior officer, Ya'ush, and in all he is apparently exculpating himself from a number of accusations. It is suggested that the letters are relics of a court-martial held on this man, immediately before the fall of Lachish. One letter appears to indicate that Azeqah, which appears to be an intermediate station between Lachish and the writer's post, has already fallen and that the writer is asking for signals to be sent direct from Lachish. This is important confirmation, though perhaps not so absolutely unshakeable as the author would suggest, that Tell Duweir is really Lachish, as Mr Starkey so plausibly insisted. But what would appear to be a weak point is the theory that in the final stage of the disaster, it was possible for the commander of the outpost, presumably after it had fallen, to evade the enemy, already masters of the intervening country, get to Lachish, and be tried by court-martial, apparently on counts extending over a considerable number of years. Another letter, III, requires a good deal of forcing to fit into the suggested chronology. It apparently refers to the fate of the Prophet Uriah, described in Jeremiah xxvi, who fled to Egypt, but was brought back and killed. Jeremiah described this as occurring in the reign of Jehoiakim (608-597 B.C.), whereas to fit into the theory propounded for the letters, it must have occurred about 590-588 B.C., in the reign of Zedekiah. This Professor Torczyner explains by saying there must have been a scribe's mistake in copying the relevant passages of Jeremiah, but he has to do some rather elaborate explaining away of certain points, which at times savours somewhat of special pleading.

But, though there are undoubtedly points which will not convince everyone, Professor Torczyner has exhibited much skill in elucidating

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a connected narrative from these letters, which may quite well turn out to be absolutely correct, if for the moment some suggestions must be held to be not proven. In any case, the presentation of all the evidence is admirable, and there is no doubt at all that we have here a most important contribution to the history of the last days of the Judean kingdom.

The remaining works under consideration are of slighter character. The one on Petra, however, is a most interesting survey of that remarkable place. The romance of this desert-capital of the caravan state of Nabatene, carved mainly out of rock in a stately natural amphitheatre, whence the trade of the Near East was controlled in the period immediately before and after the beginning of the Christian era, has tended to hinder a scientific approach to its problems. A thorough examination of the city was of course undertaken by Brunnnow and von Domaszewski, but their interpretation was not always sound. Mr and Mrs Horsfield's survey is extremely valuable. The volume under review is only part of the whole, and it is therefore difficult to criticize it, for what appear to be omissions may be remedied in later parts. The first section gives a useful summary of the topography of the place, which should be comprehensible even to those who have not visited the site. The authors suggest that the original acropolis was on Umm el Biyara, with a settlement in the Siyagh at its foot, but it must be confessed that they can only adduce probabilities to support this, and archaeological evidence is badly needed. The second section deals with the houses, which is very welcome, since few earlier writers have realized that though the carved façades belong to tombs, the hundreds of other cavities are really houses from which the masonry fronts have fallen away. The various types of houses are described, and one of them is dated to between 70 and 30 B.C. by the style of wall-painting. This emphasizes the chief need of Petra—the scientific dating of the various structures by archaeological methods, for a survey alone is not enough. Perhaps this is to come in one of the subsequent sections. The survey is illustrated by a great number of photographs of great merit. When most of the work is of such a high standard, it is a pity to include an inferior one such as LXXII 2.

Evidence of the widening of the field of Palestinian Archaeology is given by the 'Annual' of the American Schools of Oriental Studies, for it is concerned with a survey of Edom. This is as it should be, for the isolation of the Jews from their contemporaries simply because of our interest in their religion and literature gives us only an unbalanced

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picture of them. The survey of Petra, just considered, is concerned in the main only with the later Nabatean period of the principal city of the country, and it is important that the earlier part should also be considered. The present survey is only a superficial approach to the problem, and was not presumably intended to be any more. Much undoubtedly can be done by the collection of surface sherds, but two definite reservations must be made. In the first place, a number of test-sites require to be excavated in key positions in order to establish any close classification of pottery, and this is still lacking in most of Southern Transjordan. In the second, such a rapid tour as the one undertaken by the American School cannot hope to be exhaustive, and the evidence of surface finds from most sites tends to emphasize, perhaps exclusively, the latest date of occupation of the site. Both these points may affect the conclusions drawn from the tour, which were that there were periods of intensive occupation from *c.* 2200-1700 and *c.* 1300-800, with a blank between and after. Many Palestinian archaeologists will want proof from excavations that part of the Middle Bronze and Late Bronze periods are in fact not represented in Edom, for which there appears no adequate historical reason.

The work suffers most, however, from its method of presentation, which is that of a journal. Little adequate idea is gained of the general geography of the country and an analytical approach would have enabled a far clearer conception to be gained of the settlements of the different periods. A mass of irrelevant detail is given, notably the hour on which the party set out each day. It is surely of very little interest to read 'March 23rd. We left et Telah at 8.00 a.m., going due west. The area traversed at first was a waste land of grayish earth and sand covered with stones and boulders. At 9.00 a.m. we entered a golden, sandy area, tinged with green, with a very sparse growth of grass and shrubs and small flowers with white blossoms . . . A cool breeze blew during the early part of the morning and the ride was the most pleasant experienced during the entire expedition'. After some 123 pages devoted largely to details such as these, it is rather tantalizing to have the undoubtedly interesting subject of pottery dismissed in 14 pages, with the remark, 'Because of limitations of space, a fuller discussion than is possible here of the pottery collections made by the writer from numerous Early Bronze Age sites in Moab and Edom must be reserved for another occasion'. The pottery is illustrated by drawings, and by photographs, the latter of which would be much improved by not being taken in full sunlight. But though this report does not go very

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deep, it does show what a wide field of investigation lies in southern Transjordan, and it is much to be hoped that the American School will follow it up with a more exhaustive examination.

The remaining work, *Excavations at Sepphoris*, is, unfortunately, an example of how excavations should not be done. It is, it is true, called a preliminary report, and it is possible that omissions will be rectified in a fuller report, but as it is issued six years after the excavations were carried out, it would not appear that we can expect this soon. But one cannot feel great hopes when the excavator states that 'Sepphoris, like all large and important sites that have been extensively built over in the Hellenistic and Romano-Byzantine periods, shows practically no stratification'. This statement shows a reversion to the period when archaeology in Palestine was only evolving. As long ago as 1890, the excavation of sites by stratigraphical methods was started by Sir Flinders Petrie on the brick-built sites of southern Palestine. But the excavation of the stone-built hill-towns of northern Palestine, with the deep-cutting foundations, and still more with extensive stone robbing, was a very different matter. When excavations such as those at Gaza and the first excavations at Samaria were carried out shortly before the War, technical methods had not advanced sufficiently far to disentangle the disturbed from the intact levels. But nowadays there is no reason why a site in Palestine should not produce just as much stratified evidence as one in England or anywhere else. The history of the site, which is first mentioned by Josephus, from literary sources, is well outlined, but it must be confessed that the excavations have added little to our knowledge of it.

But, though this last book is an exception, the other works discussed do show the healthy state of Palestinian Archaeology before the present troubles brought everything to a standstill. The new Rockefeller Museum at Jerusalem encloses collections of great importance for the history of mankind from the earliest periods down to the present day, and scientific excavations with which most of these books are concerned have made it possible for these to be minutely and accurately classified. Both general and Biblical history have gained by the more balanced approach which is now being made to the problems of the country, the almost inexhaustible interest of which is shown by the fact that new discoveries can range over such a wide field as from the earliest Stone Age to the correspondence of a Judean commander besieged by the Babylonian armies.

The Roman Camp-Site near Castor on the Nene

by CHRISTOPHER HAWKES

IN his Editorial Notes for September 1930, the Editor of *ANTIQUITY* described an exploratory flight over England and Scotland taken by him and Mr H. J. Andrews in June of that year, in the course of which a most remarkable discovery was made.¹ A main objective of the flight was the Roman town of Durobrivae,² where the Ermine Street from London to Lincoln and the North crosses the river Nene, now the boundary here between the counties of Huntingdon and Northampton. Extensive, and for their period careful excavations were carried out in this vicinity by E. T. Artis in the twenties of the 19th century, and illustrated in a sumptuous quarto volume of plates³; but despite exhaustive summaries of these and all recordable discoveries contributed to the *Victoria County History*, by Haverfield for the Northamptonshire side in 1902,⁴ and by Miss M. V. Taylor for the Huntingdonshire side in 1926,⁵ no systematic modern work has been attempted on any part of the site, and an air reconnaissance seemed an ideal way of re-introducing it in 1930 to the attention of archaeologists.

The village of Castor, whence comes the modern name of the pottery that was the Roman town's chief industrial product, lies on the north of the river in Northamptonshire, largely, it seems, on the site of what was in later Roman times at all events the central inhabited area of Durobrivae. But what appears to be the primary urban site, traditionally named 'The Castles', is a mile away⁶ on the south bank, in the

¹ *ANTIQUITY*, 1930, IV, 274-5.

² For the degree of probability that this name in the Antonine Itinerary is indeed the correct one, see *V.C.H. Northants*, I, 166-7; *V.C.H. Hunts.*, I, 262-3.

³ E. T. Artis, *Durobrivae* (1828).

⁴ *V.C.H. Northants*, I, 166 ff.

⁵ *V.C.H. Hunts.*, I, 228 ff. This account includes the results of numerous observations by Mr Wyman Abbot, F.S.A.

⁶ Cf. the Roman milestone found by its north gate, giving 1 mile as its distance presumably from what was reckoned the centre of Durobrivae: *C.I.L.* VII, 1156 with *Eph. Ep.* IX, p. 634; *V.C.H. Hunts.*, I, 234-5.

ROMAN CAMP-SITE NEAR CASTOR ON THE NENE

Huntingdonshire parish of Water Newton. This is an irregular but compact area enclosed by a rampart and ditch, with Ermine Street running

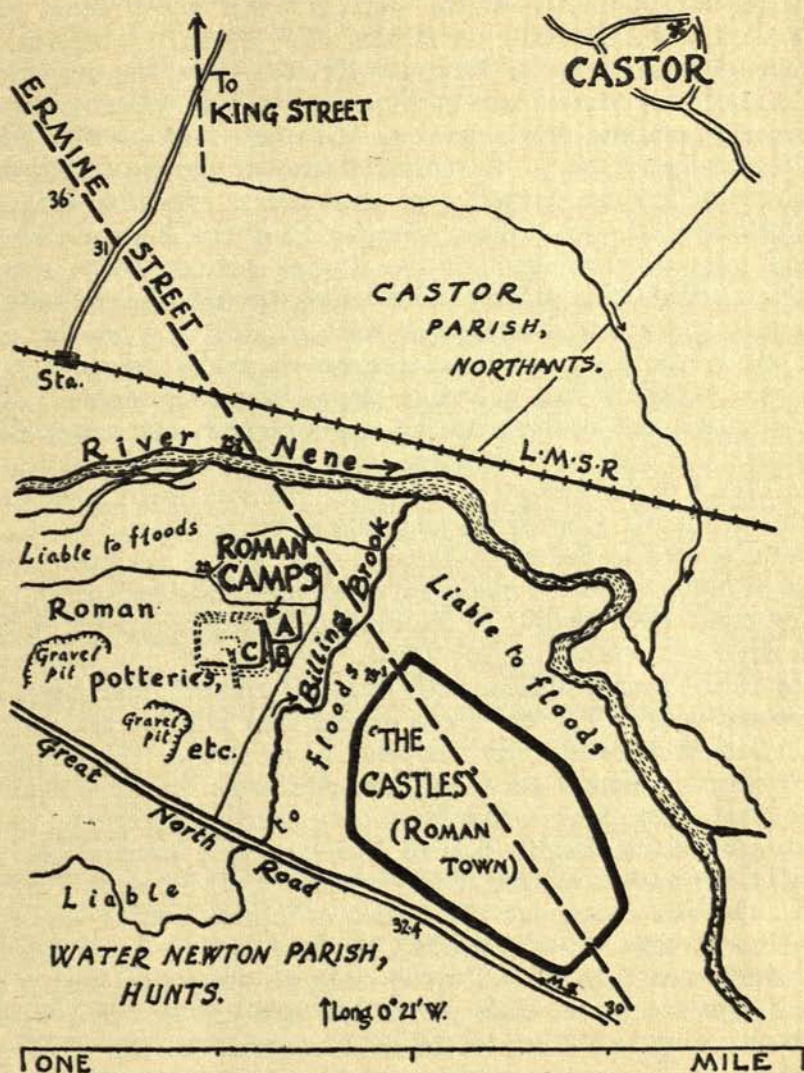


FIG. 1. SKETCH-MAP SHOWING THE CASTOR ROMAN CAMP-SITE

straight through it from southeast to northwest, just north of the milestone on the modern Great North Road marking 5 miles to Stilton and 9 to Stamford (FIG. 1, from 6-in. o.s. Hunts, II, NE). The aviators

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saw the Roman town-plan here, with its streets and some of its houses, plainly outlined in the corn. But also, a little further to the northwest, across the rivulet known as the Billing Brook which flows past the town's west wall, they saw something else, similarly outlined, which had remained unknown to Artis and all other earth-bound observers. It was the plan of a fine Roman military camp, 'complete with rounded corners, and, on the north side, no less than four parallel ditches'. Mr Crawford goes on: 'It reminded one of the fort at Ardoch in Perthshire which we visited two days later. Such a camp must surely belong to the 1st century, the period of the Roman conquest of England. Its survival through the Roman period—when the whole site was covered with pottery kilns—and through the vicissitudes of subsequent history, to be revealed thus as a shadow in the corn, is surely one of the most romantic episodes of modern discovery. It is also a new historical fact of prime importance to students'. Unfortunately it was not possible to get a photograph taken immediately, and though the discovery attracted great attention at the time, it was only on May 8 of the present year that the site was finally photographed from the air, at Mr Crawford's suggestion, by Major G. W. G. Allen. The result is seen in the PLATE facing FIG. 2 which has been prepared to show as much as can be made out of its significant features.*

The main thing visible, in the centre of the plate, is the outline of a quite large and wholly typical Roman camp (c), of the unmistakable 'playing-card' shape, oblong with rounded corners, and lying nearly due east and west. The outline is indeed very faint on the north and west, and the northwest corner appears to lie outside the picture, but the two eastern corners are clear, the southwest one just detectable, and the plan emerges accordingly as very nearly a rectangle, of mean internal dimensions roughly 600 by 450 feet, and internal area about 6 acres. This outline is that of the innermost of the camp's defensive ditches. Outside it appear the traces of others. It is unfortunate that the north side, on which Mr Crawford saw no fewer than four parallel ditches in 1930, shows up so badly in the photograph; but at least one faint vestige of each of the four can just be seen, westward of the blossoming may-trees in the field-hedge at the top of the plate.

*The dimness of the photograph is due entirely to the fact that the field was planted with an unsuitable crop. It seemed better, however, to publish it now, faint as the record is, than to wait for a crop of corn and a dry year—a combination that may be long in coming again. At the time of discovery I implored Cranwell to get it photographed, but without result.—O.G.S.C.

ROMAN CAMP-SITE NEAR CASTOR ON THE NENE

The total depth of the series here would seem to be about 60 feet. On the east and south the next ditch to the innermost, running some 15 feet from it, is visible in very fair continuity; but outside of this, though portions of anyhow one more are plain in places, matters are not so clear. For on the east the course the outer lines should follow is crossed by a ditch running not parallel to the innermost, but obliquely outwards, from close proximity to it near the northeast corner to a distance of something like 60 feet outside at the southeast. There, after turning through a wide arc, it apparently aligns itself with the innermost along its south side; but this may be an illusion. For, if on the south it is only the outermost member of an aligned series of ditches, it cannot easily be supposed to follow an oblique course of its own on the east; and it appears more likely that there at least we are looking at a ditch belonging to a different camp (B), with its axis set obliquely to the alignment of the main series. In that case we have not one camp, but two, successive occupants of the same site but diverging slightly in orientation, as for example in the well-known instance of the camps on Treacastle Mountain in South Wales.⁷ And northeast of the main site, amid the blotched and patchwork markings assignable to the later Roman occupation and its potteries, appears what may very well be part of the outline of yet another camp (A), with its rounded southeastern corner close to the largest tree in the hedge-line on the right of the photograph. Northward and westward its extent remains uncertain, but its disappearance in the latter direction may be taken itself as a fact of cardinal importance, caused as it seems to be simply by the obliterating presence of the ditches of camps B and C. It means that as far as present appearances go, camp A should be the oldest of the series. Its orientation is nearly but not quite the same as that of C, while that of B is oblique to both; and B's intermediate position in age between A and C seems assured, if we accept it as indeed the vestige of a distinct camp, by the fact that it appears to truncate the south side of A, while itself truncated by the northeast corner of C. Our alphabetical order for the three apparent systems will then represent a chronological sequence of three successive camps.

Of the internal features of A it would be rash to speak, since any or all of the visible markings may belong to potteries or other remains of the later civilian Roman occupation, some of which were indeed

⁷ Wheeler, *Prehistoric and Roman Wales*, 219-220, fig. 93.

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excavated on this very site by Artis.⁸ And if B has any visible internal features they can scarcely be distinguishable from those of C. But the latter, among which Artis seems to have refrained from operations,⁸ demand some notice. The eastern and southern portions of the site show something of a system of divisions, enclosed by 'intervallum' lines running 60 feet or so inside the innermost of the enclosing ditches. And other lines seem to run from the west to the central portion, where there are clear vestiges of a complex that should represent the headquarters area of the camp. At any rate this appears to interrupt the course of the only certain road which the photograph displays. This is marked by two parallel gutter-ditch lines, some 20 feet apart, running just west of north through the ditches of the camp's south side, to stop some 180 feet farther on at the edge of the headquarters complex just mentioned. No trace of its emergence again on the north seems to be visible, and of lateral roads it is hard to speak with any confidence, though on the east there may conceivably be one along the line of the south side of camp A. At any rate this one main road into camp C from the southward is unmistakable. It cannot be called with absolute certainty coeval with the camp, for though its entry through the latter's southern defences shows the two integrated together, yet its alignment is oblique to the camp's lay-out, and rather more nearly parallel to the apparent axis of camp B. It may possibly therefore be as early as B, C being laid out subsequently astride it but with a different orientation. Its relationship to camp A, if any, is undetectable.

Now it is at once apparent from FIG. 1 that this road is not the Ermine Street that runs through the 'Castles' town-site, but a different road belonging only, it would seem, to the period of the camps. Indeed, it may well be the predecessor of the Ermine Street hitherto known. One may compare the case of the early Roman fort of Margidunum in Nottinghamshire, through which the original Foss Way, the well-known early Roman frontier-road from Lincoln to Leicester and the southwest, threads itself on a course quite different from the mathematically direct one on which it was relaid across the site in the early 2nd century, when the original defences had been razed.⁹ And there is a further reason why the known course of Ermine Street,

⁸ See his plan of the site in *Durobrivae*, pl. XXIII, and for its position, pl. 1; it lies in the north of the eastern portion, in Artis' day subdivided by field-boundaries now vanished, of the area called Conygree Close or Field: cf. *V.C.H. Hunts.*, 1, 230.

⁹ F. Oswald, *Margidunum* (Nottingham Art Mus. reprint from *Trans. Thoroton Soc.* 1927), 1 (with pl. 1), 8, 34.

ROMAN CAMP-SITE NEAR CASTOR ON THE NENE

running through 'the Castles' and across the Nene northwestwards, by Great Casterton in Rutland, and only farther on swinging north to Ancaster and Lincoln (FIG. 3), may be suspected of being later than the initial phase of military occupation represented by our camp-site. Ermine Street is not the only Roman road running from this crossing of the Nene into Lincolnshire. It is duplicated for some 30 miles by the road known as King Street (FIG. 3). This is set out on an alignment only just west of due north as far as Bourn, after which it slants off more sharply to join the Ermine Street line at Copper Hill, just short of the probable site of Causennae¹⁰ at Ancaster, while a more direct continuation of its initial alignment, with the name of Mareham Lane, runs on to Sleaford, and thence in all probability not to Lincoln but rather northeast, by Tattershall Bridge, towards the Wolds.¹¹ King Street is not now traceable until it passes out of the Nene valley and appears running nearly due north along the western edge of Ailsworth Heath, but if its general direction towards Bourn already holds good over this $2\frac{1}{2}$ -mile interval, its line will be found to start not from Castor or 'the Castles', but rather from our camp site (FIG. 1). King Street, in fact, looks like the northward continuation of the road we have seen entering camp C from the south in the same direction. If so, it will be explicable as the early or military North Road in this sector, with the Ermine Street of our maps for its civilian successor.

It is true that the lay-out of its junction with Ermine Street at Copper Hill suggests it was secondary to the latter, but this may be an illusion, especially if its objective at Copper Hill was—as it may well have been—another military camp, the predecessor of a civilian Causennae (if that was really the name) just as our Castor camp-site is of a civilian Durobrivae. Inferring the relative age of Roman roads from their apparent lay-out in ignorance of the age of the sites with which they connect may often be a rash proceeding; the change-over from an early military to a later civilian occupation of an area may anywhere have involved enough re-planning to mask the historical facts. It is only when the relative age of individual sites begins to be apparent that their accompanying roads can be historically considered. And so here the discovery of the Castor camp-site, which can only belong to the initial Roman period of military conquest and garrison, may supply the clue to the puzzling Ermine Street-King Street duplication of

¹⁰ On the question of this identification see *V.C.H. Hunts.*, I, 262-3.

¹¹ C. W. Phillips, *ANTIQUITY*, 1931, v, 355-9; *Arch. Journal.*, xci, 111 and pl. xxiii.

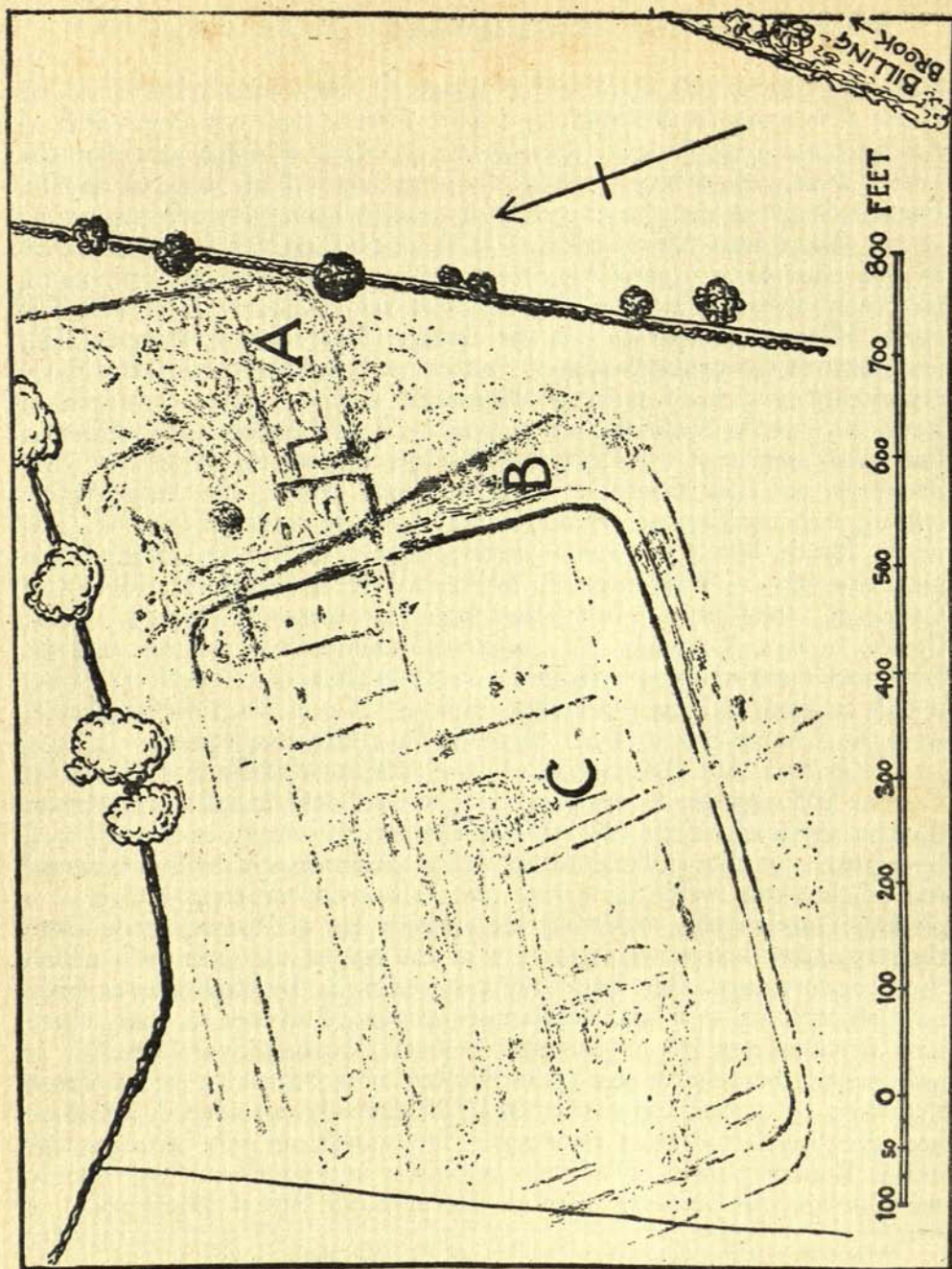
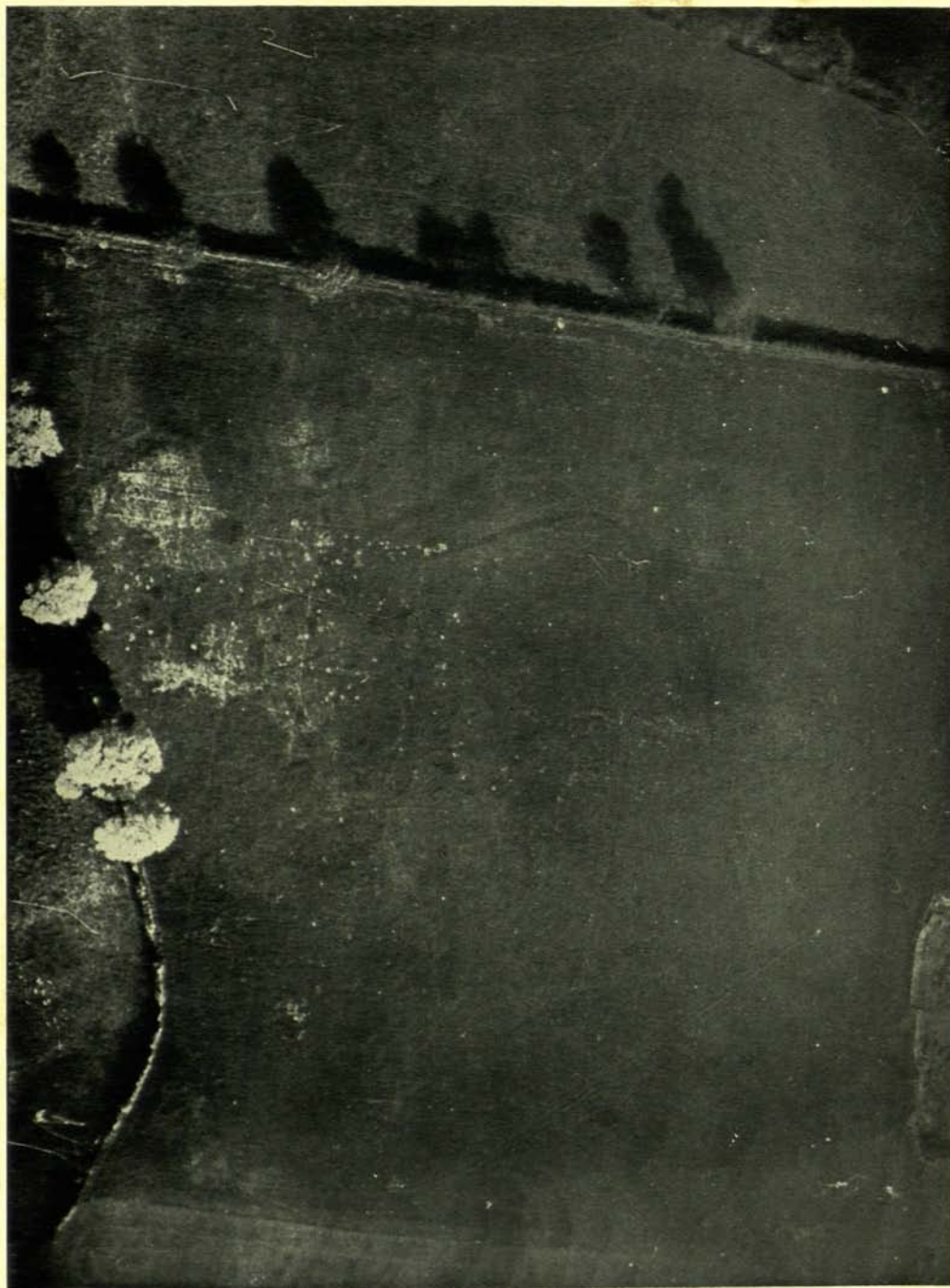


FIG. 2. SKETCH-PLAN OF THE APPARENT AIR-PHOTOGRAPH EVIDENCE FOR THE CASTOR CAMPS (cf. PLATE facing)



THE ROMAN CAMP-SITE AT CASTOR
Air-ph. Major G. W. G. Allen

ROMAN CAMP-SITE NEAR CASTOR ON THE NENE

road-plan by fixing the priority of the latter. If so, the original Roman military North Road, approaching the Nene on a line already different from the recognized Ermine Street, by way not of the civilian 'Castles' but of our newly-found camp-site, will thence have taken the King Street line, by Bourn (whence the Mareham Lane branch led off to Sleaford and the Wolds) to Copper Hill and so to Lincoln. The Ermine Street line 'Castles'-Great Casterton-Ancaster-Lincoln will then, in the form in which we know it (dotted line on FIG. 3), be the result of subsequent replanning, referable to the change from military to civilian conditions which began in the later 1st century.¹²

There is one other point in support of this notion. It is well known that the legion which formed this eastern wing of the Roman advance was Leg. IX Hispana, whose establishment in garrison at Lincoln is generally connected with the Midland frontier-line initiated by the governor Ostorius Scapula in A.D. 47.¹³ One of the pieces of supporting evidence is the tile stamped with this legion's stamp LEG. IX HISP found at Hilly Wood in the Northamptonshire parish of Ashton, which supplies, as was first pointed out by Haverfield, a pointer

¹² Though a certain number of bronze coins of Claudius have been recorded from the 'Castles' site, the main body of its coin-list begins with Vespasian (*V.C.H. Hunts.*, I, 236), and the known pottery and small finds there begin with the Flavian period (*ibid.* 248). In *Antiq. Journ.*, xv, 113-118, Mr I. D. Margary describes, with air-photographs and plans, a series of seven Roman roads in the Castor area argued by him to be later in date than Ermine Street. King Street is not among them, its line being here as invisible from the air as on the ground, but he supposes, with Codrington (*Roman Roads in Britain*, 121) that it was later than Ermine Street also. 'It seems clear' he says (117) 'that Ermine Street was the first', but surely at most only of the series he describes; while 'King Street must have been the next to be made' (118) is really a statement of opinion only, and Mr Margary's contention that the Fen Road from Denver and Whittlesey through Peterborough was planned to effect a junction with King Street and not with Ermine Street (114-5, 118) would appear to be probable rather than proved. Even if proved, it does not show King Street to be later made than Ermine Street. Mr Margary's article is a most valuable account of the civilian road-system of the Castor area in Roman times, and it is unfortunate that the failure of our camp-site to show up in his air-photographs precluded him from considering the topography of the antecedent military phase. However, it should be noticed that another camp did show up, and is to be seen in his plate XIII about 1½ miles northwest of our site; it is a rectangle with rounded corners, measuring some 330 by 220 feet (113), and though lying close beside Ermine Street, is aligned not with it but with the Roman road that runs off west to Wansford almost opposite its centre. It would be most interesting to learn the date of this camp, and its relation to those we are here considering.

¹³ Tacitus, *Annals*, XII, 31; Collingwood, *J.R.S.* xiv, 252-6; Davies Pryce, *Antiq. Journ.*, xviii, 29 ff.

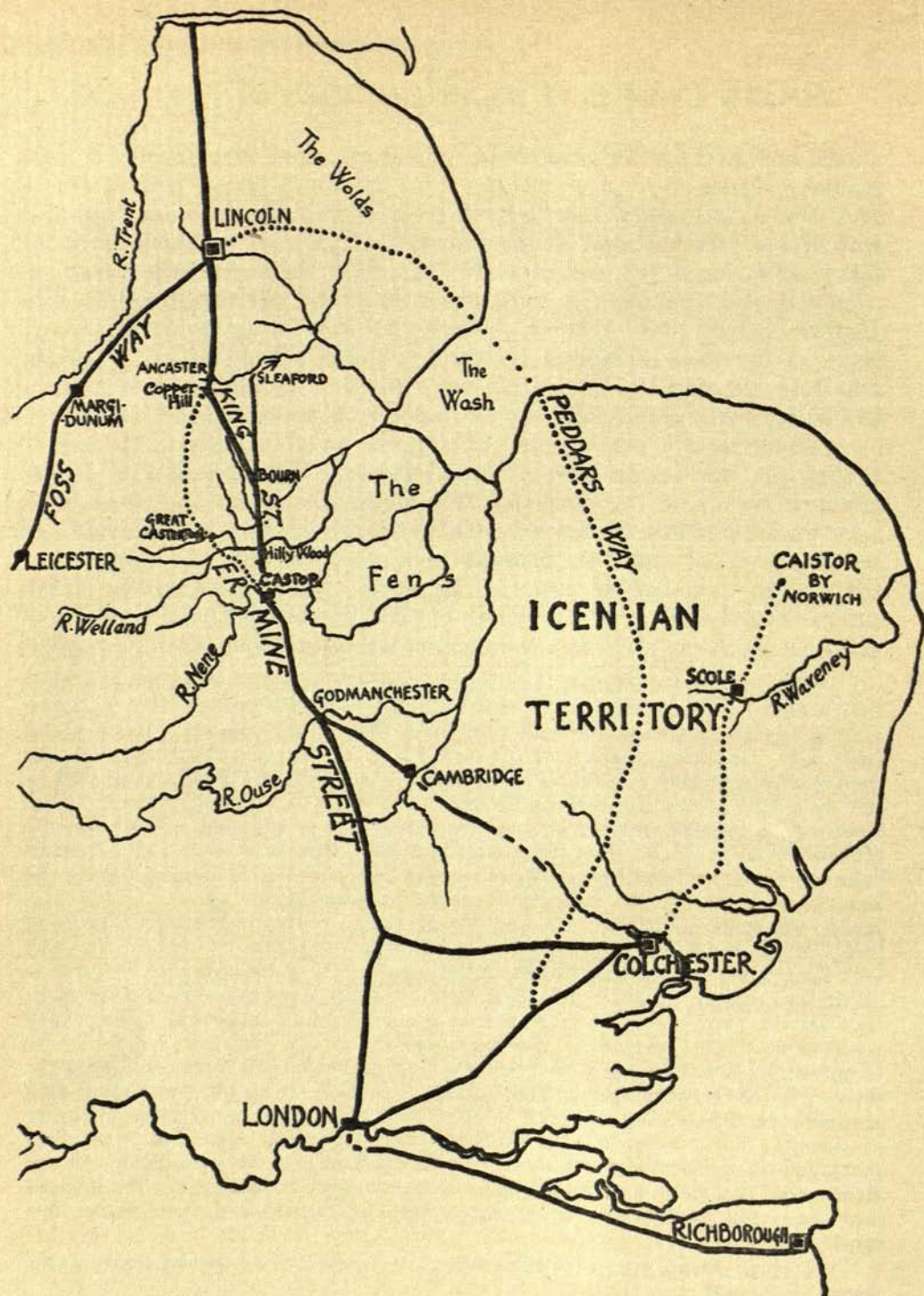


FIG. 3. SKETCH-MAP TO ILLUSTRATE EARLY ROMAN OCCUPATION IN THE EAST OF BRITAIN
Roads certainly or probably earlier than A.D. 61 in continuous line; later than A.D. 61 in dotted line

ROMAN CAMP-SITE NEAR CASTOR ON THE NENE

to its line of advance and communication from the southeast past Castor to Lincoln.¹⁴ This tile has recently been published in facsimile by Dr Davies Pryce,¹⁵ who follows Haverfield, but connects the find with Ermine Street, and marks the site on his sketch-map as west of that road's course through Northamptonshire, that is, in the parish of Ashton near Oundle.¹⁶ But there are two Northamptonshire parishes of this name, and Hilly Wood is not in that one but in the other, which lies near Helpston in the Soke of Peterborough, fifteen miles further to the northeast, and right away from the line of Ermine Street. It is in fact King Street on which this Ashton lies, about midway between the Nene at Castor and the Welland, which it crossed at Lolham Bridges,¹⁷ and its course actually forms the boundary of Hilly Wood, which is furthermore such a narrow strip of woodland that the tile must have been found in very close proximity to the road.¹⁸ The Hilly Wood tile is therefore evidence that the early military road between Castor and Lincoln, the route of the ixth Legion, was not Ermine Street, but King Street as we have already argued.

It is to the ixth Legion, then, or to auxiliaries operating with it, that we may presumptively assign our Castor camps. Camps A and B may most probably be successive marching-camps, representing the two earliest traceable occupations of the site by Roman troops. Since the area of A cannot be determined from the photograph, we cannot guess the size of the force it housed; B is not properly determinable either, but it does not look as if its area was much greater than that of C, to which we must now return. C's internal area of 6 acres would accommodate two cohorts of a marching legion, for on the evidence of marching-camps in Scotland we may posit a scale of thirty acres to the full legion of ten cohorts.¹⁹ It is not however necessary to regard it

¹⁴ Peterborough Museum. *Eph. Epigr.* III, p. 142; *V.C.H. Northants*, I, 214-5.

¹⁵ *Antiq. Journ.*, XVIII, 46-7, fig. 3, 6.

¹⁶ *ibid.* 39 and map (FIG. 1) 32.

¹⁷ Mr C. W. Phillips points out to me that the extreme liability to floods of this crossing of the Welland would militate strongly, after some years of experience, in favour of creating the Ermine Street route as a better alternative.

¹⁸ 6-in. O.S. Northants, II, SE; Haverfield in *V.C.H. Northants*, I, 204, 214-5, and 168 with map, fig. 4. Haverfield's plea (215) for further work on the Hilly Wood site seems to have remained unanswered. He records that the tile is said to have been found 'with an empty urn', and it may have been deposited as the lid of a cremation-interment, perhaps one of a cemetery. In any case further investigation seems urgently desirable.

¹⁹ Richmond, *Arch. Journ.*, LXXXIX, 51.

simply as a marching-camp and nothing more. True, it has not the distinctive pattern of the semi-permanent camp or police-fort of the same period on Hod Hill in Dorset, which was occupied for some fifteen years or more under Claudius and Nero;²⁰ nor can we claim that it was designed for the same degree of permanence as the frontier-fort of Margidunum already mentioned, where the garrison occupation lasted as long and longer.²¹ Both these sites have an internal area of some 7 acres, whereas one may suspect that their garrisons did not exceed in numbers the two legionary cohorts which in a marching-camp only occupied 6 acres. For Roman forts for long-term garrison were made to allow more room per man than did the temporary marching camps.²² But differing as it does from these more permanent and proportionately larger-sized establishments, our 6-acre camp C yet has, in its multiple ditches, and comparatively well-marked internal features, points which imply something less ephemeral than a marching-camp *sensu stricto*, such as may be represented by camps A and B. The vital crossing of the Nene cannot have been left ungarrisoned by an advancing Roman commander so long as his line of communications across it was in any way vulnerable. And if camps A and B stand for initial temporary occupations, one would expect these to have been followed by one of longer duration, such as camp C's visible characters seem to imply.

Camp C may then have been continuously garrisoned for some little time. And if so the suitability of its size for two legionary cohorts, acceptable in a true marching-camp, will become problematic: unless they were not merely under their paper strength of 600 men but expected to remain so, they would require more room, and in any case their detachment from their legion is improbable for this sort of garrison duty, which was normally discharged by auxiliaries. If then there were auxiliaries here, the camp, though too big for a milliary

²⁰ Kendrick and Hawkes, *Arch. in England & Wales*, 214-5; Crawford and Keiller, *Wessex from the Air*, 38-41; *Antiq. Journ.*, xvii, 278 and note 2; xviii, 30 and note 1.

²¹ Oswald, *op. cit.*; Kendrick and Hawkes, *op. cit.*, 215-6.

²² The internal area of Margidunum is given as about 6½ acres by Collingwood (*Arch. Roman Britain*, 28); Dr. Oswald, the excavator, gives it as 7-8 acres (*op. cit.*, 8). The nature of the Margidunum garrison is uncertain, but one would expect auxiliaries; at Hod Hill legionaries were certainly present at some time or other, as is shown by the remains of armour and arms from the site, including the legionary *pilum*, in the British Museum. The area of the Hod Hill camp is about 7 acres, not 3.4 as implied in *Wessex from the Air*, 38, by comparison with camp D at Cawthorn in Yorkshire, which contains actually rather over 3.5 acres (Richmond, *Arch. Journ.*, lxxxix, 70).

ROMAN CAMP-SITE NEAR CASTOR ON THE NENE

cohort of them (about 800 men), which even in permanent garrison occupied only 4-5 acres, might be comfortably occupied by two quingenary cohorts (each about 480 men), or by some composite force including cavalry, possibly a *cohors milliaria equitata*, which differed from the ordinary milliary cohort in including horsemen, and may be reckoned to have required rather more space in consequence. These are speculations; similarly, the length of the occupation can only be guessed at. The stage of temporary marching-camps can hardly have outlasted Ostorius' establishment of the ixth Legion at Lincoln in A.D. 47, and while camps A and B should thus presumably belong to the preceding years under the first Claudian governor Aulus Plautius, camp C may be conjectured to date at least from that year, and must then, one would think, have been held in strength throughout Ostorius' stormy governorship, which ended with his death in 52. But it is reasonable to guess that it was given up between that date and the revolt of Boudicca nine years later, for from Tacitus' account it looks as if when the rebels sacked Colchester there were no regular troops stationed anywhere nearer than the legion in its fortress at Lincoln.²³

All these are questions which excavation alone can answer. That that may not be long delayed will be eagerly hoped by everyone interested in Roman Britain. Such opportunities, in the Lowland zone of the province, are likely to be rare. On this Lincoln route, for instance, the passage of the Ouse, where the trunk lines from London and Colchester²⁴ meet, must have been similarly garrisoned, but it is only too likely that the camp-site has been obliterated by the buildings of Godmanchester or of Huntingdon. At Cambridge, indeed, where the Colchester-Godmanchester line crossed the Cam, recent discoveries point to an early occupation in the Castle Hill quarter, which may well have been military,²⁵ but here again the ground is encumbered, as it very probably is on what was doubtless the ixth Legion's initial station under Aulus Plautius at Colchester. The Castor camps are in fact not only at present unique, but quite likely—for the east of England at all events—to remain so. Only one other suggestion of the discovery of a multiple-ditched Roman camp seems to be on record in the whole country between the Thames and the Fosse Way. This is near Scole,

²³ Tacitus, *Annals*, XIV, 32.

²⁴ The missing Colchester end of this road was recovered by excavation in 1936: *J.R.S.*, XXVII, 240.

²⁵ I owe knowledge of this unpublished material to the kindness of Mr T. C. Lethbridge, F.S.A.

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on the boundary of Norfolk and Suffolk (FIG. 3), where the Roman road from Colchester to Caistor-by-Norwich crossed the Waveney.²⁶ But the context of a camp here might well be different from that of the route to Lincoln from Colchester or London, for the Iceni of East Anglia were accepted at first as allies of Rome, and Rome, despite the trouble with them in 48, was seemingly long content to leave their territory relatively unattended to, on the right flank of the main northward and northwestward advance. In fact, it seems to have been only after this policy had had its sequel in the revolt of Boudicca in 61 that the great military road that we call the Peddars Way was made across the Icenian country,²⁷ cautiously starting on the London side of Colchester and giving communication with Lincoln and beyond by a ferry across the Wash,²⁸ and there is no reason for assigning any earlier date to the road from Colchester to Caistor by way of Scole.²⁹ It is then very possible that a military camp in this direction may date only from the morrow of the revolt, antedating by no more than a decade the establishment of civilian life among the Iceni in Romanized towns and villages, which began in the Flavian period.

At Caistor, on the other hand, if our conjectured diagnosis of this photograph is at all correct, we have three camps, of which A and B seem quite likely to belong to the period *c.* 45-47, while C may itself be as early as the latter year, and may yet very probably, after an occupation of moderate length, have been abandoned already during the '50's. Even if this prove too sanguine an estimate, the place is unquestionably a pre-Flavian military site, to which an early Claudian beginning cannot reasonably be denied. Its encumbrance by later potteries, etc., seems from the photograph to be less serious than one might have feared. Could there be a clearer case for excavation? Here is a chance for opening quite a new chapter in the archaeology of Roman Britain.

²⁶ *Proc. Prehist. Soc. E. Anglia*, I, 321-3; *Proc. Suffolk Inst. of Arch.*, XXII, 269.

²⁷ *Norfolk & Norwich Arch. Soc.*, XXVI, 153-9.

²⁸ *ANTIQUITY* VI, 342-8.

²⁹ *Norfolk & Norwich Arch. Soc.*, XXVI, 161-2.

The Winding Road

by F. G. ROE

IT has often appeared to the writer to be a really remarkable circumstance, during the long and sometimes acrimonious controversies of the nineteenth century concerning the various physical, social, and 'technical' phenomena (methods, etc.) characterizing the Saxon settlement of Britain and its more immediate antecedents, that among the various protagonists of the first rank, none seems to have thought it worth while to visit those lands where an essentially similar environment still prevailed, and to see for himself what they might yield. To make such a statement concerning archaeological students of today would certainly be to invite questions in return, which are not easily answered. What *was* the (physical) environment of early Saxon England; and where shall we find its 'essentially similar' counterpart? But at the time of which I speak, such doubts may almost be said to have been non-existent. Whatever opinions were held concerning Roman centuriation, or Teutonic three-field systems, the old 'traditional' view of England as 'a land of forests', 'one great wood', etc., seems hardly to have been questioned. Under such conditions, virtually any forest country occupied by settlers of European birth or descent would serve the required purpose.

The truth of this is revealed in the very language of many of the investigators themselves; uttered, one might almost say, in the very agony of their dubious peerings into the dim twilight of an irrecoverable past. Kemble wrote as follows (c. 1849):

'This state of society, if society it can be called, is rarely exhibited to our observation. The backwoodsman in America, or the settler in an (*sic*) Australian bush, may furnish some means of judging such a form of civilization; and the traditions of Norway and Iceland dimly record a similar process: but the solitary labourer, whose constant warfare with an exulting and exuberant nature does little more than assure him an independent existence, has no time to describe the course and the result of his toils: and the progress of the modern settler is recorded less by himself, than by a civilized society, whose offset he is . . .'¹

¹ *Saxons in England*, 1876, I, 67; cf. *ibid.* I, 125.

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A similar point of view is found in other scholars ; both as regards North America, and other lands, including Russia. The inclusion of Russia is of significance, in my own opinion, for more than one reason. The great Russian scholar who has done so much to elucidate many problems in this field, observed years ago :—‘ Questions entirely surrendered to antiquarian research in the west of Europe are still topics of contemporary interest with us . . . ’² My own observations in various ‘ Russian districts ’ in the forest portions of Alberta during many years revealed a fundamental similarity of method in many matters of hand-labour, where alien machine-usages at first *cannot* intervene to standardize, to those exhibited both by various settlers of the ‘ first generation ’ from Teutonic lands, and by native woodsmen of several generations’ standing, from Eastern Canada or the Atlantic States. I have long been of opinion that the broad similarity to their native environment, despite the manifest and visible certainty of years of much harder toil awaiting them than on the plains, was one of the principal attractions to the early Russian settlers in northern Alberta. The history, and particularly the chronology, of the settlement of the ‘ German state ’ of Wisconsin and the ‘ Swede state ’ of Minnesota, point to a similar conclusion. And modern antiquarian research concerning our Anglo-Saxon ancestors, with their preference for those heavily-wooded valley lands, leads one to suspect a similar incentive as a powerful influence in their case.

But despite the illumination which some of the earlier at least of these phenomena in one land or another ‘ might have possibly ’ thrown upon the problems of scholars well aware of the chance of it, none of them, so far as I have been able to discover, made any attempt to utilize it. The object of the present paper, as far as in me lies, is to draw upon this class of long-neglected evidence in the elucidation of the origins—at large—of the winding road. Much has been written at one time or another on what the old pathfinders, trail-breakers, road-surveyors, or what you will—‘ must have done ’. Some of this, coming from men who almost certainly have had no occasion to practice trail-breaking under the *real* spur of necessity, must be regarded as magnificent reconstruction. I have encountered other examples, with great names behind them, which left one wondering if the author were blind, or had ever been out-of-doors in his life ! In my earlier years in Alberta, I have both ridden and freighted by wagon over previously untracked

² Vinogradoff, *Villainage in England*, 1892, pp. v, 236.

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wastes, where the discovery of a practicable ford over a creek, or a passage across a 'slough' or 'muskeg' (*i.e.* marsh), was a matter of immediate and vital importance.

I have had to balance the various considerations, *pro* and *con*, which govern the choice of a site for the evening camp and for the permanent home. And I have seen the first faint track of some outlying neighbour—sometimes, as it were, a thing of chance; sometimes 'surveyed' through scrub and timber for permanent use as carefully and skilfully as any railroad route—grow into a well-marked trail along which (in dry weather) an automobile could travel at any speed practicable on the best of roads. In the light of these experiences, it may be of interest, perhaps also of some permanent historical value, to describe what we actually *did* do as roadmakers. It may also serve to illustrate what others 'must have done'. For men are often curiously alike in their solutions of similar problems before machinery took over the task.

The winding roads and lanes of England have given rise to much speculation at different times concerning their origin. They are in particular one of the standing surprises to the Transatlantic visitor; reared in a world of square city 'blocks' and—even before the motor-highways—straight 'concession lines' and 'road allowances'. This is not remarkable; seeing that in their own native environment many English writers and also some professed historians have either given up the problem as frankly insoluble, or have advanced semi-humorous 'explanations' which could only have been meant to hide their real ignorance. 'Curving away because the dog barked loud', in a society where the stranger's only hope lay in coming forward openly; this would involve—if taken seriously—that English roads were almost exclusively broken out by thieves and scoundrels, the very class who avoid roads as far as possible. So also, winding about 'in an unaccountable fashion';³ or, 'as if for the very sake of winding'. This would be to invest the long arm of coincidence with a power that would eclipse—and justify—the wildest effort of the romantic fictionist; when 'chance' could produce identical results in countless instances in widely-sundered localities. Apart from what seems the fundamental absurdity, *a priori*, of the corporate locomotion of any people being conditioned upon chance, the factors of wide distribution and (much) largely contemporaneous origin in my view point clearly to widely-prevalent physical

³ W. Jerrold, *Middlesex*, p. 257. He quotes also (p. 257) the verses on 'the wobbling calf'.

conditions and certain resultant principles, which could be applied whenever occasion arose.

Before proceeding further, it is perhaps not irrelevant briefly to review the question of the supposed continuance on a general scale of Roman (or pre-Roman⁴) road systems unchanged in alignment down to the present day. It is not necessary to subscribe to the fantastic supposition of Roman Britain as a perfectly-organized Roman polity, fully equal in every phase of social and 'mechanical' expression of civilization with the Eternal City itself; and handed over to the Anglo-Saxon invaders in unimpaired 'running order', much as one engine crew turns a locomotive over to another. It is nevertheless possible that in some places the modern (*i.e.* pre-motor) principal English roads may actually lie for material distances on the very same alignments as their Roman 'predecessors' between the same general points. But there are some strong conflicting considerations against this as a general truth. Even as the political subdivisions of Roman Britain corresponded in no single case with the later English 'shires', so also, many of the Romano-British cities were more or less distant from the (roughly corresponding) local capital of the district in later times. Even so little as a mile or thereabout, as at Verulam—St. Albans,⁵ might be sufficient to necessitate great changes, under certain topographical conditions.

A great authority has stated that no instance was known of the invaders utilizing any deserted Roman villa as a home.⁶ This would inevitably entail the abandonment of many Roman 'roads'—or tracks—and the striking out of others to the more favoured sites. Some of these might be utterly obliterated in a fairly short time by cultivation; others may possibly persist to this day as 'green roads', or those 'drovers'

⁴ Rice Holmes speaks of 'the trackway on the line of which the Romans at a later period made the great road called Watling Street' (*Ancient Britain*, p. 344). Dr Williams-Freeman considers that 'the pre-existence of a British track along the direction of a Roman road seems to be the rule, rather than the exception . . . (*Field Archaeology of Hampshire*, p. 220). The Icknield Way is a British trackway; so, A. Mawer, *ANTIQUITY* I, 152; but apparently never 'Romanized' throughout in construction. Watling Street (north of London) and other 'Roman roads' not British 'intertribal tracks' in Haverfield's opinion: *Eng. Hist. Rev.* ix, 725; cf. *ibid* xi, 428-30. Later, to Rice Holmes, 'I know nothing satisfactory about the line of Watling Street, and nothing to suggest that it existed before A.D. 43 . . .' (Holmes, *op. cit.* p. 705).

⁵ See *ANTIQUITY*, 1938, xii, 16-25.

⁶ Haverfield, *Roman Occupation*, p. 274.

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roads' or 'packhorse ways';⁷ which doubtless survive in living memory as alternative ways between various points and London, along which men might perhaps find better fare for their beasts than along the recognized high roads, while also evading turnpike dues. I have myself encountered wayside indications of other road-systems than those now in use in that particular locality. There remain also two factors of prime importance; and I believe that the latter in particular very often fails to receive due recognition. These are the lapse of time between *permanent* Roman and Saxon occupation, and the moist English climate.

Haverfield was inclined to antedate the Roman 'abandonment' of Britain to A.D. 407, rather than 442, as preferred by Bury;⁸ and the loosening of the Roman grip may have begun sixty years before that. I consider that public works of this character are precisely those which begin to suffer first in any society which regards its future as precarious. Security of possession is a fundamental incentive toward maintenance and upkeep; and among causes and effects of social disintegration, one of the commonest is the impairment of the financial machinery which provides for such matters. Recent historians are recognizing that the era of 'Roman Britain'—for whatever it may be worth—must at least be extended down to the time when something like an orderly English local administration of affairs had had time to shape itself.

We have here a series of physical phenomena, which it seems fair to assume were exposed during the larger part of two-and-one-half centuries to almost entire neglect, together with the natural ravages of a highly unfavourable climate. We have practically nothing to oppose to the consequences which such premises would logically suggest, except some rhetorical platitudes about the indestructibility of Roman

⁷ See Cox, *Green Roads of England*, 1922. Oman thinks that 'in some cases they made shift to employ the old tracks during the whole period of their occupation of Britain'. (*Eng. Before the Norm. Conq.*, pp. 80-1). No doubt the Icknield Way would be an outstanding instance. We may, I think, be sure that the by-road to every Roman villa was no Watling Street. Mr Belloc talks of the puzzling 'disappearance of Roman roads all over the country': (*Warfare in England*, p. 80). I doubt if there were as many 'Romanized' roads to disappear as he seems to think. Tacitus mentions British complaints of 'difficult cross-country roads' (manifestly in contrast to better main ones: *Agricola*, CXIX; cf. *ibid.* CXXXI). It is against all economic and administrative usage to suppose such distinctions would not persist; and the disappearance is not puzzling, except to those who substitute their own intuitions and prejudices for research.

⁸ *Roman Occupation of Britain*, p. 157; cf. *History*, x, 325.

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roads, 'lasting for ever'. Nothing lasts for ever, not even a Roman road; and the greater the resistance, the greater, inevitably, must be the force which finally overwhelms it, and its ruin (as an integral whole) the more complete.

The supreme enemy of all roads is water. Robert Stephenson laid it down as a principle that standing water must not be allowed to approach within less than three feet of the grade surface of a railway.⁹ Though not a civil engineer, I have done a good deal of grading work on 'dirt roads' where we had to provide for drainage with nothing but our own experience and such material resources as the woods might furnish. In such conditions, it does not take long to recognize the danger-spot on one's road. This, in an irregular country, is where the topographical surface-contour changes from being below your road to above it, or from 'fill' to 'cutting'; necessitating a long ditch through this entire section, or passing the drainage under your road to follow a natural fall—perhaps both. At one precise spot, your drain and the topographical surface are absolutely level with each other. This is the exact place where free out-fall must at all times be maintained; and experience shows it to be the one where it is most difficult to maintain it. At this point, the ditch (if it be unlined) changes from its soft clay bottom to encounter the stubborn sod of the surface with its tough grass roots impeding the flow of the water and sedimentary silt. A chip, a small tuft of grass or leaves, a pebble, the silt washed down by heavy rains and slowly settling as it passes along the ditch—particularly as the volume of water decreases and the current slackens in consequence; and before very long the water is 'backing up' and running *over* the road instead of beside or beneath it, and its certain destruction is only a matter of time if no relief comes, Roman road though it may be. It is almost impossible for us even to conceive of any process of neglect and decay continuing for two centuries, in anything which during that time *still had to be used*; as England's ruined abbeys or castles—almost the only parallel example—had not. Our own knowledge of similar havoc during less than a tenth of the time makes it quite clear, however, that the picture has not been overdrawn. I have no doubt at all that in numbers of cases those portions of Roman roads which enjoyed natural drainage from traversing hilly country would be passable for traffic long after it had become extremely difficult to use those going through low and marshy ground. It would probably

⁹ F. S. Williams, *Our Iron Roads*, p. 114.

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be at the points I have indicated—where surface and ditch-levels were identical—that the road would first break down under the attempts to struggle across. Each rut in the gradually disintegrating surface would hasten decay in an accelerating ratio; and a moment's reflection will show even an unpractised man how utterly useless a long stretch of excellent road may be, if a narrow gap renders it almost impossible to get on to it or off it again. Under such conditions, I believe the early English settlers would be glad in many places to break away from the marsh or depression and go round.

Dr Cunningham¹⁰ wrote, many years ago :—

(In Gaul) . . . 'the Roman civilization had so utterly decayed that the soil had to be reclaimed again, by clearing the forest and jungle . . . ' But in England, . . . 'so soon as commerce developed, the old Roman routes of communication were ready prepared for the chapmen who began to traverse them, and Roman bricks lay ready to hand for the repair of Roman bridges and the construction of new towns where the Roman cities had stood . . . '

So far as physical forces are concerned, I cannot comprehend his distinction between the two countries. A vast mass of evidence tends to indicate a general physical resemblance; and Britain was probably the wetter. Another (contemporary) statement about 'Roman roads which the English adopted and kept in repair',¹¹ will not, in my view, bear to be pushed farther than that the English would use them when they could, and had to do something to them in order to be able to use them. I do not consider the mere existence of a law—the famous *trinoda necessitas*—as any sort of historical proof that Roman roads and bridges were maintained in any reasonably practical engineering sense.

For I think it may be laid down as a sound general proposition, that the successful maintenance in any real, homogeneous sense, of any highly-perfected technical production such as was a Roman road, involving an advanced degree of engineering knowledge and large resources of labour, is virtually impossible except to those possessing similar skill and equal resources for its utilization. I have met with no evidence to indicate that the English settlers possessed either. Roads built like the great main Roman highways will undoubtedly stand up longer than a mere trail; but when they do begin to give way, their

¹⁰ *Growth of Eng. Industry and Commerce*, 1910, I, 67; *ibid.* I, 59, 96-7. The modern conclusions appear to be almost the precise opposite: see R. G. Collingwood, *ANTIQUITY*, I, 117-19.

¹¹ Elton, *Origins Eng. Hist.*, p. 339.

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ruin will be the more utter and complete if there be but the trail-maker's resources wherewith to repair them.

This is not mere conjecture. Here in Alberta, following upon railway amalgamations, many miles of steel have been taken up along lines no longer considered necessary, owing to duplication or other reasons. The abandoned grades have largely been utilized as highways. It very speedily developed that the rough-and-ready methods which had served to keep 'dirt roads' in (comparatively) fair condition for years would be valueless on a gravelled highway; maintenance must imperatively follow along the same lines as the original construction. The most unimaginative, untravelled stay-at-home could readily conceive the futility of filling a 'hole' (*i.e.* a slight, travel-worn depression) in the Great North Road or the Watling Street, with a few 'scraper-loads' of soil or clay, as would be done on a dirt 'road'.

There is no lack of evidence, in my opinion, both for the (apparently rapid) decay of Roman bridges and for the diversion of routes, in part, from Roman roads. If Roman bridges as a whole had lasted well into the Anglo-Saxon era before disappearance, as phenomena which the English had seen in some plenty before their fall, should we not have had more English place-names of the 'Pontefract' type? (*ponte fracto*). The tenacity of names needs no labouring. There is a well-known place-name in this very field, running into scores of specimens. I refer to the countless something '-ford Bridges'. These (including many on 'Roman roads') go back for long centuries; and all the changes of the years have been unavailing to eliminate the now superfluous 'ford' name. Living recollection in some cases, and common sense, apart from the structure of the word, tell us that the *ford* is older than the *bridge*. Why then does not a similar harvest of 'Pontfords' or 'Brigford Bridges' preserve a memory of those (Latin) bridges which were older than the (English) fords? They adopted 'street' in plenty for an unfamiliar phenomenon; why not 'bridge'? Bridgford, Nottingham, and one or two others, *may* perpetuate this sequence; but they are very few.*

* *Leland's Itinerary* mentions 73. But the annual volumes of the Eng. Place-Name Soc. have already yielded 152, from twelve counties only. I give the earliest date in each. Beds.—Hunts. 6 (1165); Bucks. 1 (1250); Devon 17 (1242); Essex 41 (12th cent.); Herts. 14 (1324); Northants 10 (1221); Surrey 20 (1140); Sussex 13 (1347); Warwick 16 (1225); Worcs. 5 (1229); E.R. Yorks. 7 (1066); N.R. Yorks. 2 (1230).

See editorial remarks *in re* Brushford (*brigeford*), Devon, Somerset, in *Devon* (Eng. P.N. Soc.), II, 361. There is Briggswath (*wath*=ford), N.R. Yorks. (*id.*) p. 119 (in Whitby).

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I consider that a distrust of Saxon engineering science being equal to the repair and maintenance of Roman masonry is justified by the extremely rude construction of many bridges in much later times. The most important in all medieval England, London Bridge itself, was of wood until 1176. Up to as late as *c.* 1650, timber bridges are stated to have been common, south of the Trent.¹² In the medieval period and later, there are numerous grants of timber for the repair of bridges; and incidental evidence reveals how rude some of them must have been. Sometimes they were mere logs or planks thrown across a stream. We find the abbot of Coggeshall (Essex) in 1307, refusing to maintain bridges on the ground that those whose upkeep devolved upon his predecessor were merely of that primitive character.¹³ The well-known incident of the Earl of Hereford, speared from beneath at Boroughbridge in 1321—itsself fore-acted in a similar occurrence at Stamford Bridge in 1066—shows that the bridge must have been of rude construction, with wide cracks in the flooring. The periods are too late to be of much significance; but both places were on Roman roads.

In relation to the question of the unchanged alignments of ancient roads on the routes they now follow, as against possible diversions, the appeal to old maps, showing the same towns on the same roads as today, and the citation of historical references to the same effect, has apparently been thought a satisfactory, if not unimpeachable criterion. 'Ancient road' is a vague and elastic term. In most cases I believe such a claim might be conceded for *post*-Conquest highways; apart from some of those minor alterations like the Pastons made, or the actual shutting up of local roads for short distances in the later inclosure era. The 'Something-ford Bridge' argument clearly implies (and requires) this. But if the conception is to be indiscriminately pushed back to Roman times it behoves us to be cautious. The towns could not very well be moved; but their immobility would not necessarily preclude route-diversions at intermediate points, if occasion arose. We actually possess historical evidence of such in many localities. Coming, as it mainly does, from the older English topographers and antiquaries, it is additionally valuable; since the possibility cannot arise in their

¹² Joan Parkes, *Travel in Eng. in the Seventeenth Century*, 1925 (with some interesting particulars on decay of ancient bridges), pp. 28-34. Leland (*Itinerary*) gives 117 of timber, 371 of stone, 222 not specified.

¹³ Jusserand, *Wayfaring Life in Mid. Ages* (ed. 1909) pp. 63-70, 417-18.

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day of a Roman highway having been diverted to allow a railway to pass over it. Camden describes one :—

‘ More east, the Roman road leads strait from London to Verulam over Hampsted heath through Edgeworth and Ellestre¹⁴ . . . When the Roman power ceased here and barbarism crept in during the confusion of the Saxon wars this road was like others long neglected till a little before the coming of the Normans, Leofstan, abbot of St. Albans restored it . . . He cut down the woods which reached from the edge of the Chilterns to London, particularly by the high road called Watling Street . . . But 300 years since, this road was in a manner deserted, and another opened by leave from the bishop of London through Highgate and Barnet¹⁵ . . . ’

Any philosophic doubt concerning the foregoing would seem to be dispelled by old William Harrison’s relation concerning the Watling Street, near St. Albans, in 1531¹⁶ :—

‘ . . . the course thereof was found by a man that digged for gravell where with to mend the high waie. It was at this place eightene foot broad, and about ten foot deepe, and stoned in the bottome . . . and peradventure also on the top : but these are gone, and the rest remaine equall in most places, and leuell with the fields . . . ’

Harrison further describes the Watling Street as ‘ still winding in and out ’ from near St. Albans to Caxton, Huntingdon, Stamford, Torksey, and onward. This can scarcely be dismissed as mental aberration ; for in one of the countless discussions on the meaning of *Watling* (now happily settled, may we hope¹⁷), Camden or his editor suggests the name is ‘ perhaps derived from its winding nature (? “waddling”) ’ ; ‘ but it is certain that this does wind most of any of the four grand ways ’¹⁸ . . . One does not unduly emphasize a modern holiday writer’s allusion to ‘ a sudden kink in the remarkable straightness of Watling Street ’ ; for it is no doubt taken for granted that the Northampton-Birmingham road *ipso facto* is ‘ Watling Street ’ of Roman fame. But there are allusions by scholars to other Roman roads which are not direct. And it may be noted that among those regarded as being so, the later tendency is to interpret this as a ‘ point to-point ’ alignment—whether *de novo* or as an ‘ ironing-out ’ of the minor irregularities in preceding tracts ; which may be considered as a

¹⁴ Edgware, Elstree.

¹⁵ Gough’s *Camden* (ed. 1789), I, 339-40 ; cf. Leland, *op. cit.* v, 151.

¹⁶ W. Harrison, *Description of England* 1577, ed. F. J. Furnivall, 1877, III, 145.

¹⁷ English Place-name Society, *Herts.*, pp. 7, 86-7 ; *Beds.* pp. 5-7.

¹⁸ Gough’s *Camden*, I, LXXIV ; cf. Gough’s remarks *in re* Camulodunum (*ibid.* II, 56-57). He notes Watling Street ‘ running E and W ’ near Penkridge, Staffs. (*ibid.* II, 385).

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natural rebound from the over-emphasis on the *absolutely* straight Roman road of some writers—even late ones. We have also quite clear indications of 'Roman roads' where the more solid construction or foundation—in some recognizable form—encountered *lacunae* every now and then, either on straight, apparently homogeneous sections; or in a manner suggestive of diversions no longer readily perceptible. In other cases, the modern highway, after following for a distance the (supposed) Roman alignment, 'keeps straight on'; while the road of actual (former) Roman construction 'turns aside' toward some ancient objective. And yet in others, the ancient way, under some old local designation, is at one time following its time-honoured alignment through the heart of some historic English town; and again—bearing the same name—is far from the modern highroad of the district, across the fields.

It is extremely probable that that citizen of St. Albans, in 1531, knew where to look for his gravel; and the same circumstance may help to explain those countless other diggings in highways (or ex-highways), which seem otherwise so utterly foolish. For I doubt whether the most individualistic and irresponsible of men would select that hard, solidly-packed ground, except for something he believed he could not obtain elsewhere.¹⁹ The cumulative effect of the foregoing evidence seems to be that the 'traditional' view of the uninterrupted use of Roman ways cannot stand. They were used where topographical, climatic, perhaps economic conditions favoured this; in other conditions, they were partly or wholly abandoned, or utilized as quarries, or even forgotten altogether; while the English settlers marked out new or 'substitution' routes to suit themselves, on the readily recognizable principles of a forest folk.

I am not disposed to belittle the importance and value of the topographical study of the English country-side. I regret that my own opportunities, during forty-five years' absence, have not been greater. Perhaps by reason of this, I may tend to exaggerate the difficulties which there confront the out-door student who lacks the immense facilities of aerial investigation. The factors of more intensive cultivation, the network of railways, drainage, the vast increase in the size and number of towns, the artificial alterations and modifications which these have occasioned, the miles of high walls around great houses which prevent

¹⁹ This was the successful defence of the miller of Aylesbury, 1499, for the death of an unfortunate traveller; cf. Norwich, 1507: A. S. Green, *Town Life in the Fifteenth Century* (2 vols. 1894), II, 31-2.

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examination of many of those very regions most likely to have preserved their physical characteristics with the least change, the hedge-rows which often intercept the view in level regions, and many other later features, seem to have so essentially altered the ancient appearance of the land, as to make its imaginative 'reconstruction' a rather difficult task. But Alberta can help us here.

Apart from the relatively few who appear to be congenitally and hopelessly lacking in any sense of location, it is astonishing how soon a town-bred stranger learns to find his way about in a new land. The 'eye for country' and its accompanying 'trail-instinct', appear to develop naturally. In the latter respect, particularly, we in Alberta had excellent tutors; for the few trails in the country had been broken out by masters in their art. Fortunately they were unhampered by the later fetters of the 'road allowance'. In open country, they sought the ridges and clung to them as long as possible; if not on the very 'comb', far enough up to ensure dry firm ground. In scrubby and even quite thickly wooded country, they reduced a preliminary clearing with the axe to an astonishing minimum. If there were anywhere near a thinner patch of timber through which pack-ponies, carts, or wagons might worm their way along, keeping a general direction very closely—as likewise do the winding English roads—but none the less twisting this way and that around big trees, thickets, bogs, or windfalls, those old woodsmen and trail-breakers might be trusted to find it, and with an absolute minimum of soft ground. It was not until the country became more thickly settled and fenced, confining us to the straight road allowances, that we really saw much of bad roads.

When a small creek or strip of marshy ground simply had to be crossed, the driver swung and picked the likeliest-looking place—the trail there being usually 'ten trails' (or more) wide; and when—or if—across, he swung back into the common track once again, which made for the higher land as speedily as possible.²⁰ And thanks to the dry intervening sections of 'contour' trail, our horses were in much fresher condition approaching these 'muskegs' (which, if really bad, we 'brushed' or 'corduroyed'); and we were seldom 'badly stuck'.

²⁰ 'The roads (in Eng.) were much like an up-country trail at the present time in Alberta, or in the Great North West, where the traveller "breaks trail" as he goes along, avoiding the worst places by the simple expedient of going around them': H. S. Bennett, *The Pastons and their England*, 1922, p. 129. cf. Duignan ('Saltway'), *Worcs. Place-names*, 1905, p. 141. On Sept. 27, 1929, along the valley-road, Baslow to Bakewell (Derby), the ancient duplicated paths along the 'side-hill' were clearly visible.

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Apropos of these trails, from 1899 to 1903 we experienced a cycle of five wet seasons.²¹ The use of the fenced road-allowances was forced upon us by settlement; yet very little grading or ditching had been done. In our black-soil scrublands, the absurd straight road which looks so impressive on a map, broke down under the strain. In numerous cases, the road allowances were abandoned to the farms adjoining them; and winding 'contour' roads were surveyed on the precise principles of the old trails, and permanently expropriated by the Dominion Government.

In many cases, the English winding roads reveal causes for their windings, essentially unaltered through the years, of a similar general character to the foregoing. Perhaps a small hollow in a field, which in pre-drainage days would be a 'pot-hole' of oozy mire, dangerous perhaps to man and beast; around which the road prudently skirts. A hillside wood, which probably enough from soil and situation may never have been aught else since England existed. Or the circuitous approach to a ford, where the direct 'attack' might be too steep to get up (or even *down*), or might face some deep reach unsuitable for fording. I have seen 'ford Bridges' in England where despite the later raised bridge-'causeway', the long winding approach of the 'ford' days has remained unaltered, to reveal its history. But there are also others, less plainly self-evident.

When the inevitable use of the road-allowance in the near future began to be clearly foreseen, it became our habit, when forced by the fencing up of some old trail to select a new route to our local market, to endeavour to utilize the still unfenced road-allowances as much as practicable. There were then no road-grading machines in the country, either horse or automotive power. We trusted entirely to traffic to wear our new trail smooth—it did not take very long on a main route—and we begrudged the labour of a new 'cross-country' trail if its fruits were to be taken from us at a moment's notice. Many of these road-allowances ran through tall, thick scrub. We made no attempt at first to clear the entire width of 66 feet; in many instances we could pass along entirely by winding from one side to the other, picking where the scrub thinned out, exactly as did the older trail-breakers; and never attempting to take a heavy wagon through the centre of a large clump of willows, which would have had to be cut out for the purpose, and

²¹ I learned to swim in 1901, in a small 'lake', eight feet deep in the centre; out of which we hauled the hay, in full loads, in 1898.

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under which was a soft, yielding 'leaf-mould' unable to sustain any weight. It very often happened that the soft, rich, black loam—the joy of the cultivator and the despair of the roadmaker—being shielded by the tall scrub and trees from the full rays of the sun, and badly cut up by the rains and the traffic, became almost impassable. In such conditions, a common expedient was to clear the wood-growth across the entire width of the road-allowance, and allow sun and winds free access to the surface; and unless the season was abnormally wet, we speedily had a dry firm road, generally much better than a 'graded' road which had destroyed the solid surface of sod and brought the clay subsoil to the top. The remarkable result for my present purpose is that the windings, which, while the great thickets that had occasioned them were still standing, appeared to be the very quintessence of selective skill and balanced trail-judgment, seemed the height of absurdity when there was no longer any visible reason for them; presenting the precise appearance afore-mentioned, of 'winding for the very sake of winding'.

We have here in a nutshell the life-history of the winding English road, along those countless expanses of level or gentle slope where it is evident no permanent physical feature remains to indicate a cause. Even in heavily-timbered countries trees do not invariably stand on the ground like a wall. The characteristic form of the common tree of medieval England, the oak, with its short stem, and low far-spread branches, seems to have been so uniformly pronounced as to give rise to a generic name not applied to other trees.²² Whereas our Alberta trail-windings are rather minute and sudden, deflections around smaller trees or thickets having no wide overhang of shade, I believe that in countless cases the sweeping generous curves of the English roads would be found to correspond quite closely with the circumferential radius of some huge branching oak. Perhaps even travellers on horseback could not be sure of 'clearance' overhead beneath the central boughs. It seems perfectly certain that no loaded wain would attempt such a short cut, even if not burdened with a high load of hay or sheaves. For there would be a sodden, sunless, almost mossy surface, which nothing but long-continued drought could render

²² The 'brood oak' or 'brod ooke' in Coventry (1410, 1423, etc.) which gave its name to 'Brodewok' or Bradock Waste (*Coventry Leet Book*, I, 18, 46; II, 439, 440). The well-known tree-valuation in the *Laws of Ine*, 'by the number of swine which could stand beneath', points to wide-branching trees. See further notices, ANTIQUITY, x, 338.

THE WINDING ROAD

fit to sustain a load ; with the additional risk of striking some huge radiating root, over which no team could lift a wheel. As against this, the curve around the outer ' rim ', even if the tree and its next neighbour almost touched, would at least get whatever sun there was.

So long as the English who (I believe) made them, ruled the land, it is not probable that there existed at any time among so conservative a race, much desire to ' iron out the kinks '. It is not difficult to reconstruct with strong probability the later conditions which would tend to settle them so firmly in their winding alignments that even the revolutionary era of Macadam wrought little change.

The ' technical ' requirements of the great Statute of Winchester (1285) are not original. In 1250, the woods on the highway between Coventry and Warwick were ordered to be cut down for a space of ' Six Acres in breadth ' (66 feet = one acre ; hence 396 feet²³). The later law repeated this, making it applicable at large ; and failing obedience, compensation for injuries should fall upon the landlord whose non-compliance had furnished shelter upon his land to the evil-doers. We may—I think—assume a certain degree of compliance, under strong kings at least, with a law which entailed such disagreeable consequences on its violation. And the incidental physical benefits of obedience might attract some who disdained its obligations.

The first effect of the clearing of the undergrowth for a space of 400 feet would be similar to what I have described in the case of our Alberta roads. The sun and the winds would keep the way in far better condition than before. This would considerably reduce the need for ' alternative trails ', except in very bad spots ; and would tend to fix one particular track as *the* road. I believe the consequent result would be that the lord, finding himself under the necessity of keeping a certain portion of his land free from scrubby growth, would do what most of us would do under similar conditions—he would keep it cleared by the very effectual, the only permanently effectual method, of cultivating it ;²⁴ perhaps even the more readily, since pecuniary legal

²³ In Dugdale ; cited by C. Holland, *Warwickshire*, 1908, p. 168 ; W. H. Hutton, *Highw. and Byw. in Shakespeare's Country*, p. 380.

²⁴ It may be urged that such crops, when well grown, could shelter robbers just as easily. Yet there apparently *were* such, perhaps of fair size : ' Ich haue an half-acre to eren by the hy-weye . . . ' (*Piers Plowman*, ed. Skeat, *C. passus*, ix 2). ' Half-acre ' = a small piece of ground (so, Skeat, *ibid.* ii, 106, note) ; but it has been noted in contradistinction, that a large number of people are set to work ' ploughing it ' : (*N. and Q.* 5 S. ix, 347). The diminutive might be used ironically, as often now.

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consequences might fall upon himself, while the labour would devolve upon underlings. This would confine the roads with all their original sinuosities to the space they already occupied and no more. The road would be the boundary of the fields on either side. We may be quite sure that in any later Crown expropriations for the widening of highways, the centre of the road would be the *datum* from which the desired width would be taken, half on either side. The mutual jealousies of rival landowners would ensure that English conservatism could be trusted to do the rest.

Donegal Survivals

by E. ESTYN EVANS

RECENT articles in *ANTIQUITY* have drawn attention to the rich sources of material for study in ethnology and archaeology which lie almost untapped in the dwellings of peasant Britain. For Ireland three papers by Åke Campbell¹ break new ground in a virgin field and increase the heavy debt which Irish scientific studies owe to Scandinavia; and the interest roused has been fostered by a short statement on the need for enquiry into house-types issued by Colonel R. G. Berry.² The Irish Folklore Commission has gathered much information on this and related topics, while a regional survey of the house-types of north Kerry, clearly inspired by Campbell's work, has lately appeared.³ The participation of continental workers in this field is further illustrated in the 'Contributions to the study of the tangible material culture of the Gaeltacht' published by L. Mühlhausen.⁴

The familiar statement that Ireland is a storehouse of the past, preserving, it may be said, the secrets and the ethnological treasures of northwestern Europe, is substantiated by these enquiries. The regions of Ireland which have retained the Gaelic tongue—the remote western peninsulas—have naturally kept many other ancient culture elements, and it is to the Gaeltacht that the student first turns his attention. In the north of Ireland the culture-lag, already apparent in the northeast, increases towards the west: the centuries fall away as one approaches the Atlantic, and to journey from east to west is to travel into the past.

The district I am concerned with lies about the great quartzite cone of Errigal, between the barrier range of the Derryveagh Mountains and the Bloody Foreland. It is along this coast, from Tory Island southwards, that the most primitive types of sea-going currachs are found—

¹ 'Irish Fields and Houses', *Béaloideas*, 1935, v, 57-74; 'Notes on the Irish House', *Folkliiv*, 1937, i, 207-234; 1938, ii, 173-96.

² 'Irish Long-houses'. *Irish Naturalists' Journal*, June 1938, 63-4.

³ K. Danaher, *Journ. Roy. Soc. Ant. Ireland*. December 1938, 226-40.

⁴ *Journ. Cork Hist. and Arch. Soc.* 1933, 66-71; 1934, 4-51.

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the paddle curraghs—while the rowing-curraghs⁵ also retain features which those of other coasts in western Ireland have lost. But the isolation is most marked inland, among the peat wastes where the wheel was almost unknown a generation ago. Here, under Errigal, are communities which might be called archaeological fossils, preserving the past and living on the past, dependent on the turf bogs not only for fuel, as in most parts of Ireland, but also, until recently, for all constructional timber in the form of bog-‘fir’ and -oak. One feels that the Bronze Age culture which was adapted to the margins of those living forests still survives, in essentials, on their ruins.

There can be little doubt that many of the cultural features to be described were at one time widely distributed in the north of Ireland. We may recall that in prehistoric times, as in the historic period, successive waves of culture reaching Ireland from overseas penetrated at various entries on the northeastern coast between Carlingford Lough and Lough Foyle. The present political boundary of Northern Ireland illustrates this, and also emphasizes the southwestward thrust to control the passage between Lough Erne and the Upper Lough. In the north of Ireland the dominant structural trend is from northeast to southwest, and this trend guided penetration and retarded infiltration into the remote northwest, which was thus late in receiving and late in losing new ways of life. The megalithic civilization of Donegal, for instance, is an impoverished and no doubt retarded reflection of the virile forecourt-cairn culture of the northeast. It is in Donegal that we should expect to find traces of the cultural heritage of ancient Ulster, both Celtic and pre-Celtic.

Among features which may well be pre-Celtic in origin is a system of openfield cultivation (rundale) which centuries of opposition from landowners have failed to destroy. I have described this survival elsewhere,⁶ and need only draw attention here to the fact that, on the Donegal evidence, rundale cultivation was associated with nucleated settlement and that the scattering of the habitations with their long ‘strip farms’ was, in this part of Ireland, a process which took place mainly in the 19th century. Some writers on this topic have assumed or asserted that rural settlements in Ireland have always been of the *Einzelfhof* type—single farmsteads surrounded by their enclosures. The

⁵ J. Hornell, *British Coracles and Irish Curraghs*; [a review of this book will be published in an early number of ANTIQUITY.—EDITOR]. Estyn Evans, ‘The rowing-curraghs of Sheephaven’, *Ulster Journ. of Archaeology* (1939), 28–31.

⁶ ‘Some survivals of the Irish openfield system’, *Geography*, March 1939, 24–36.

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occurrence of ancient one- or two-field systems of communal cultivation⁷ in many parts of western Europe and the Mediterranean supports the suggestion that its origins in Ireland are prehistoric and pre-Celtic. Another practice formerly common in Ireland and found in many upland regions in western Europe and the Mediterranean lands is pastoral transhumance. The movement of cattle and of most of the population from lowland villages to mountain shielings or 'booleys' survived into living memory in Donegal, and its influence may still be traced in many aspects of Irish rural life; in the rights of common hill grazing frequently enjoyed by farmers, in the inadequacy of permanent dwellings which once had to serve for winter only, and perhaps especially in the attachment of the Irish people to the festival of Halloween, the season when the hill-folk returned, still marked by its family reunions and much feasting.

In the area under consideration several hamlets have escaped the disintegrating force of consolidation, or farm-'squaring'. The members of the community hold their cultivable land, which is unenclosed, in scattered strips or plots. In these hamlets and in the larger rundale villages formerly prevalent the dwellings are loosely gathered around the water supply (PLATE I). They may be roughly aligned along a road (cf. Teelin, southwest Donegal, PLATE II), but they are never strung end-to-end as in the numerous small market towns of the Irish countryside. Each dwelling has the air of ignoring the existence of its neighbours, and in the dense clusters of the 19th century new houses were evidently built where they could be squeezed in. Where there is free choice, shelter from the westerly gales appears to be the first consideration in the selection of a dwelling-site. I have heard it said that one method of choosing was to let the wind decide by throwing up a hat on a stormy day and noting its resting-place. The house, as a rule, turns either its back or its top gable (behind the hearth) to the west. A bedroom, behind the hearth, benefits from the warmth of the chimney-back. Although it is or has once been present in all the dwellings I have examined it does not appear to have been an integral part of the structure; the original dwelling was in fact extended as family-increase made further accommodation desirable, and with a declining population the 'room' has now often been allowed to decay. The bedroom is never at the end opposite the chimney: it was considered unlucky to extend the house at this, the bottom, gable.

⁷ S. Harris, 'Some notes on field systems', *Sociological Review*, July 1928.

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Some of the houses have been improved and enlarged in different ways in the last half-century, and many are now being replaced under the Government housing-subsidy scheme, which at the same time enforces the destruction of the old dwelling or its conversion into a farm building. Fortunately it is still possible to find a few examples of what must be regarded as the basic house-type, the constructional unit in its simplest form, a gabled rectangular building having hearth, bed and byre under the same thatched roof.

The custom of providing space for cattle under the family roof seems to have been prevalent over northwestern Europe. It occurs in areas of heavy rainfall where the emphasis is on dairy produce rather than on crops, and where easy access to the cattle in the storms of winter was necessary. The practice is certainly ancient in Ireland, and while many explanations and apologies are offered by different writers ('the cow keeps the house warm', 'it makes it easier to get a drop of milk', 'warmth increases the yield of milk', 'it helps to save the dung') it was tenacious tradition rather than poverty which determined the long survival of the custom.

The bed occupies a specially built recess near the hearth. The byre-end of the building is no longer used for cattle; it has generally been boarded off to serve as a dairy, store or bedroom. I have found only one house in the Errigal district, at Meenacreevagh, which is still inhabited and in its original condition (FIGS. 1, 2, 3, 6). An older dwelling nearby (FIG. 4), now a stable, is said to be nearly 200 years old. Most of the houses are more recent: it is in style, not in age, that they are old. Externally they are, if somewhat drab, characterized by a fine simplicity. 'Lacking nearly every architectural consciousness and at the same time every kind of imported building-material, the Irish peasant house never stands out in bold relief against its background, but melts into it even as a tree or a rock'.⁸

The names given to the two sizes of dwelling house recognized—the 'two-couple' and 'three-couple' house—indicate the paramount importance of the coupled rafters which support the roof. The wooden framework of the roof not only determines and limits the size of the room but is the chief pride of its owner. The country is almost bare of trees, and until lately all the roof timber was obtained from the bogs. The buried trunks of pines are located in winter by seeking frost-free patches (the distribution of dew may similarly be used as a guide in summer) and extracted after testing their condition with a probing rod

⁸ Campbell, *Folkliv*, loc. cit., 222.

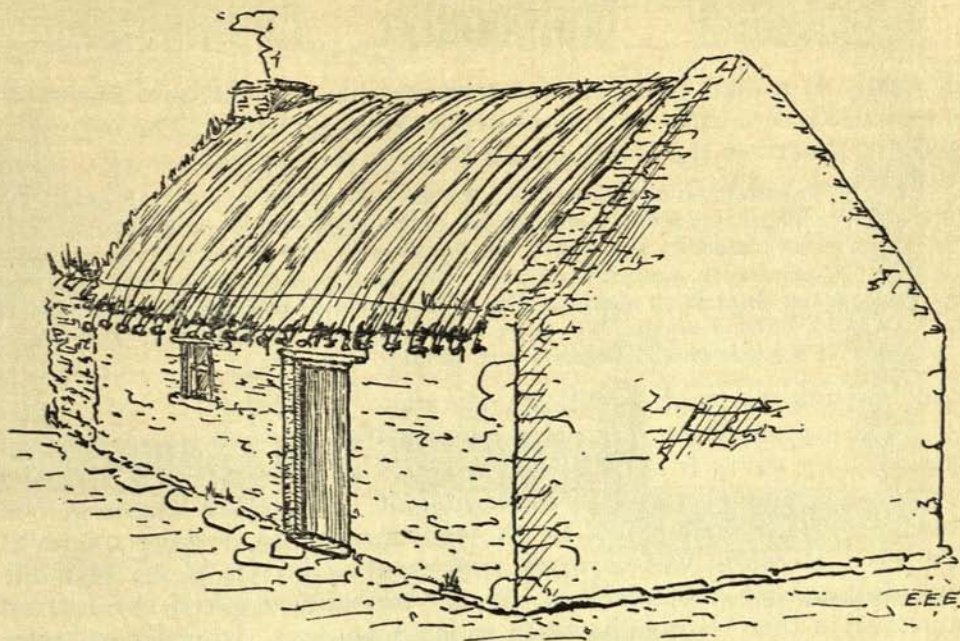


FIG. 1. BACK WALL AND BOTTOM GABLE OF KITCHEN-BYRE, MEENACREEVAGH (HOUSE A)

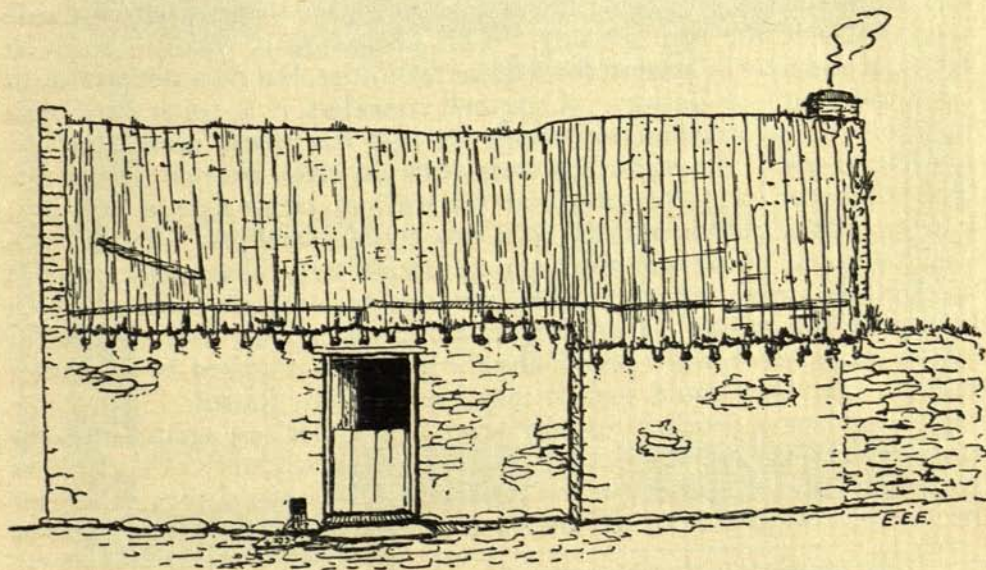


FIG. 2. FRONT ELEVATION OF KITCHEN-BYRE, MEENACREEVAGH (HOUSE A)
 Bed-recess on right, and drain by the door

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of iron. If a man contemplated marriage his chances were improved if he had his roof timber gathered before seeking a wife. The following account describes the erection of a house a century ago :—

‘ The custom on such occasions is for the person who has the work to be done to hire a fiddler, upon which engagement all the neighbours joyously assemble, and carry, in an incredibly short time, the stones and timber upon their backs to the site ; men, women, and children alternately dancing and working while daylight lasts, at the termination of which they adjourn to some dwelling where they finish the night, often prolonging the dance to dawn of day, and with little other entertainment but that which a fiddler or two affords ’.⁹

Much communal work is still done at such ‘ gatherings ’.

The approximate internal dimensions of the two-coupled house are 22 ft. by 12–13 ft., of the three-coupled 25 ft. by 13–14 ft., but the foot is not used as a unit of measurement. While the length of the dwelling could be increased by the addition of one or more rooms at the gable, its width was narrowly restricted by the size of the available roof timber, and experience of disaster probably inspired the current superstition that ‘ it is unlucky to widen a house ’. In order to house the testered bed and leave an open space around the hearth a small ‘ bed wing ’ is attached to one of the side walls, projecting some two feet externally (FIGS. 3, 4, 6) ; the first couple of the roof bears on the side wall alongside the bed opening. This projection is widely known in Donegal and may be seen in the adjacent counties, but does not appear to be found south of Galway. Campbell considers it a north European feature.¹⁰ The wooden bed which fits into the recess measures 6 ft. by 3 ft. 6 ins. ; the straw mattress rests on cross bearers of bog-fir. The other roof couples spring from the walls on either side of the door, but the central pair is omitted in a two-couple dwelling. Two cross-beams secured by trenails tie the couples together. The couples do not meet at the ridge, which is thus rounded in a stream-lined curve to throw off the wind. This device, moreover, allows short beams to span a wider space, while preserving the steepness required by the heavy rainfall, than they could span if they were directly joined.

Long beams (purlins) usually seven in number rest against wooden pegs driven into the couples (FIG. 5), and the framework is completed by laying laths of split fir on the purlins. All the woodwork is heavier on the windward side of the roof and is made lighter on the sheltered

⁹ Lord George Hill, *Facts from Gweedore*, 5th ed. (1887), 41.

¹⁰ Cf. the wall-beds formerly found in the Hebridean black houses and shielings. E. C. Curwen, *ANTIQUITY*, September 1938, 261–89.

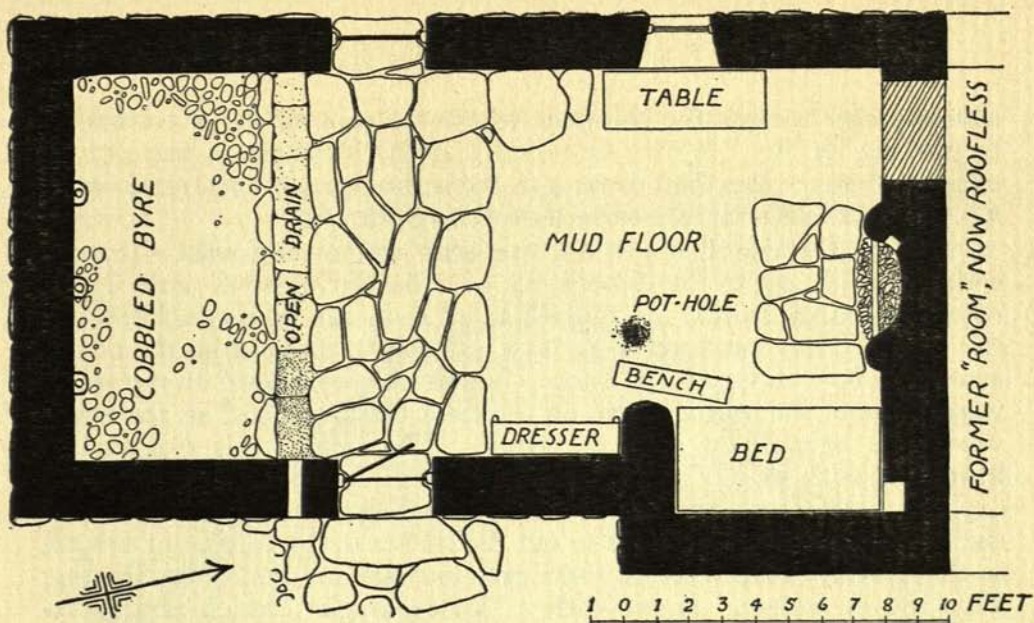


FIG. 3. PLAN OF HOUSE A, MEENACREEVAGH

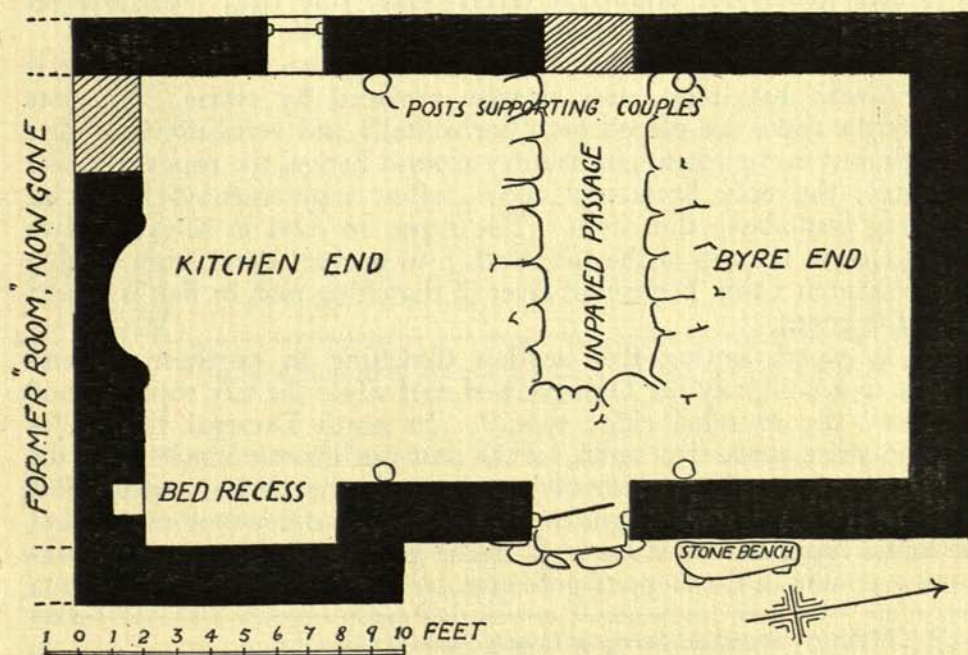


FIG. 4. PLAN OF HOUSE B, MEENACREEVAGH, NOW USED AS A STABLE

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side and also towards the chimney, where the peat smoke is considered to prolong its life. It may be noted that the lower cross-beam of the couples is never less than some 9 ft. from the floor, giving ample room for the manifold activities carried on inside (FIG. 6).

Before the thatch is put on, the laths are covered with a layer of sods, carefully cut to the dimensions of a measuring-stick and cleaned of surplus earth, so that the rough matted roots can be pressed between the laths. They measure, so far as I was able to judge from the inside, some 6 ft. by 2 ft. by $1\frac{1}{2}$ ins. thick. Three sods are placed on the windward slope of the roof and two on the lee; the upper sod on the windward side being bent over the ridge. The upper sods overlap the lower and each strip of sods overlaps the next, care being taken that the exposed edges face away from the prevailing wind-direction. It is the custom in county Tyrone to cut the turves up to 20 feet in length, so that a single strip will run from eave to ridge and bend over the top. This sod or 'scraw' is cut with a 'knee-plough', which is a short-handled breast-plough. A similar implement is still to be seen in the Antrim glens, and the Tyrone form was no novelty in 1800,¹¹ a fact which makes one doubt the claim (*Man*, July 1933, 116) that the breast-plough was an English invention of the 18th century.

The thatch, of 'sprit' or of flax specially grown for the purpose, is tied on with hay rope, now usually replaced by twine. In some districts the ropes are placed both horizontally and vertically in a close network, but in the mountain country around Errigal the ropes run over the ridge, the only horizontal ropes, often supplemented by laths, occurring just above the eaves. The ropes are tied to rows of stone pegs placed at the top of the side walls; to prevent them from sinking into the thatch a thin horizontal layer of thatching rush or flax is placed under the ropes.

It is worth noting that scollop thatching in northern Ireland appears to accompany the high-pitched roof while the hay rope method goes with the rounded ridge type.¹² In north Donegal the gables commonly rise above the thatch, for the roof-purlins rest on a ledge in the end walls. It would be difficult to secure the horizontal ropes. But elsewhere the thatch runs right over the gables, and this type usually has horizontal ropes tied to stone pegs in the gables. An interesting compromise results in the stepped gable (PLATE III), an artistic exaggeration

¹¹ J. McEvoy, *Statistical Survey of County Tyrone* (1802), 51.

¹² Campbell, *Folkliv*, loc. cit., 228.

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of the profile of the gable, which allows the horizontal ropes to pass through the angles of the steps.

The walls of the older houses are cemented with mud, but have been pointed and faced with mortar and limewash within and without. The thickness is found to be 20 inches in all cases, the eaves rising to 6 or

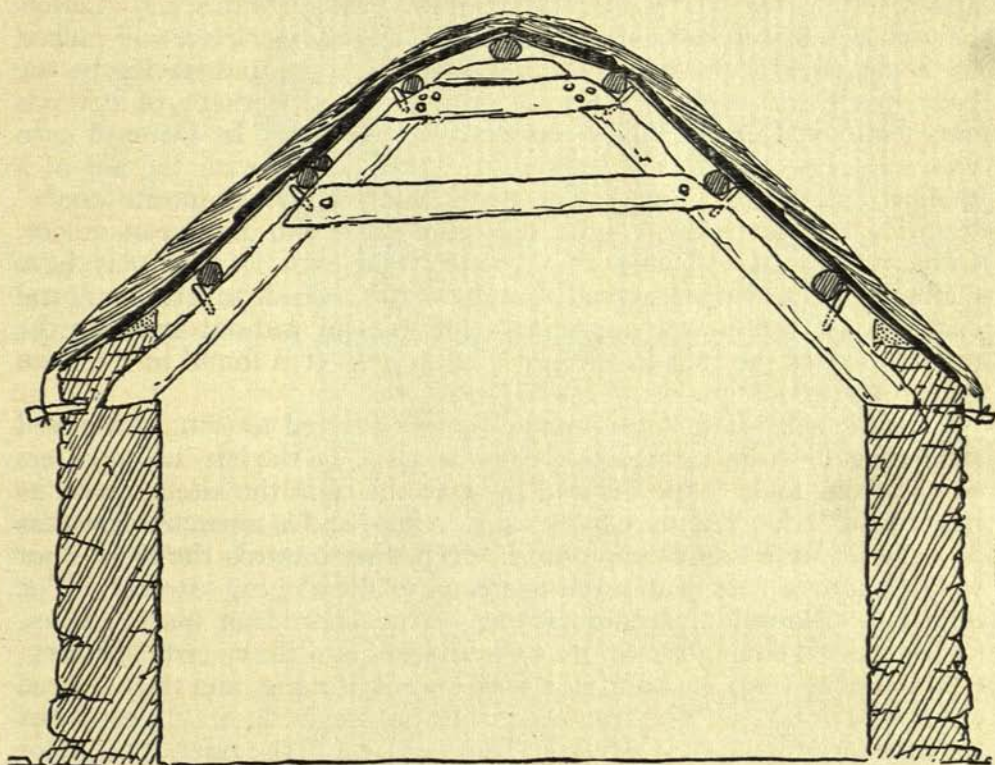


FIG. 5. CROSS-SECTION OF HOUSE IN ERRIGAL DISTRICT, DONEGAL, SHOWING ROOF CONSTRUCTION

7 ft. and the gable ends to 12 or 13 ft. Mud walls are not used in this area, nor are sod houses (PLATE IV) now to be found.

Most of the dwellings have, or once had, two doors facing each other, at back and front. Although the back door has gone out of use it is sometimes still in position, and in other cases the opening can be traced blocked up or converted into a window or, as I have seen it in county Louth, disguised as a recessed wall cupboard. Much superstition attaches to the back door, and long tradition seems to lie behind

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it. One reason given for its disuse is that strangers, more frequent nowadays than formerly, might leave the house by a different door from the one which they entered and so take the luck of the house with them. It is clear, however, that the back door has outlived its functions, which were connected with wind protection and smoke disposal and also with the care of the animals, as in the Welsh long houses.¹³ Campbell records that in parts of the west of Ireland the cattle are still milked in the house, each animal entering by the front door and leaving by the back, but that this is done in summer only,¹⁴ a relic, perhaps, of methods once employed on the hill pastures at that season. In Donegal corn was sometimes winnowed between the open doors with the aid of a shallow circular skin tray. The distribution of the 'opposite doors' tradition suggests that it is an old west and south European device. Climatic conditions along the Atlantic fringe of Europe may have facilitated a northward spread, but it would be rash to make cultural correlations without archaeological support, and without knowing the distribution of the trait in the rest of Europe. It is found in southern Scandinavia.

In the inhabited single-roomed house referred to (FIG. 3) a paved strip $5\frac{1}{2}$ feet in width connects the two doors. To the left, as one enters by the front door, is the disused byre, to the right the kitchen with its hearth and bed. The byre has a cobbled floor and is separated from the paved walk by an open stone drain, deepening towards the front door where a square hole in the wall opens on to the sloping 'street'. The dung was allowed to accumulate as in the Hebridean black houses, 'some houses having within its walls from one to thirty cwts. of dung, others having from ten to fifteen tons weight of dung, and only cleared out once a year'.¹⁵ The front door, but not the back, is supplemented by an external half door which is kept shut while the open inner door admits light and air (there is no window on this side of the house). In the byre there is room for four beasts; they were tied at the bottom gable, where three perforated schist slabs built into the wall carried vertical poles separating the stalls. The widest stall stabled the horse, against the back wall, and a small wall-recess conveniently placed above the back door housed the horse brush and comb. Above the byre is a small open loft reached by a movable ladder, now used as a store but originally serving as a barn. It would hold sufficient straw and oats to

¹³ I. C. Peate, *ANTIQUITY*, December 1936, 448-59.

¹⁴ Campbell, *Béaloides*, v, 70.

¹⁵ Lord George Hill, *loc. cit.*, 17.

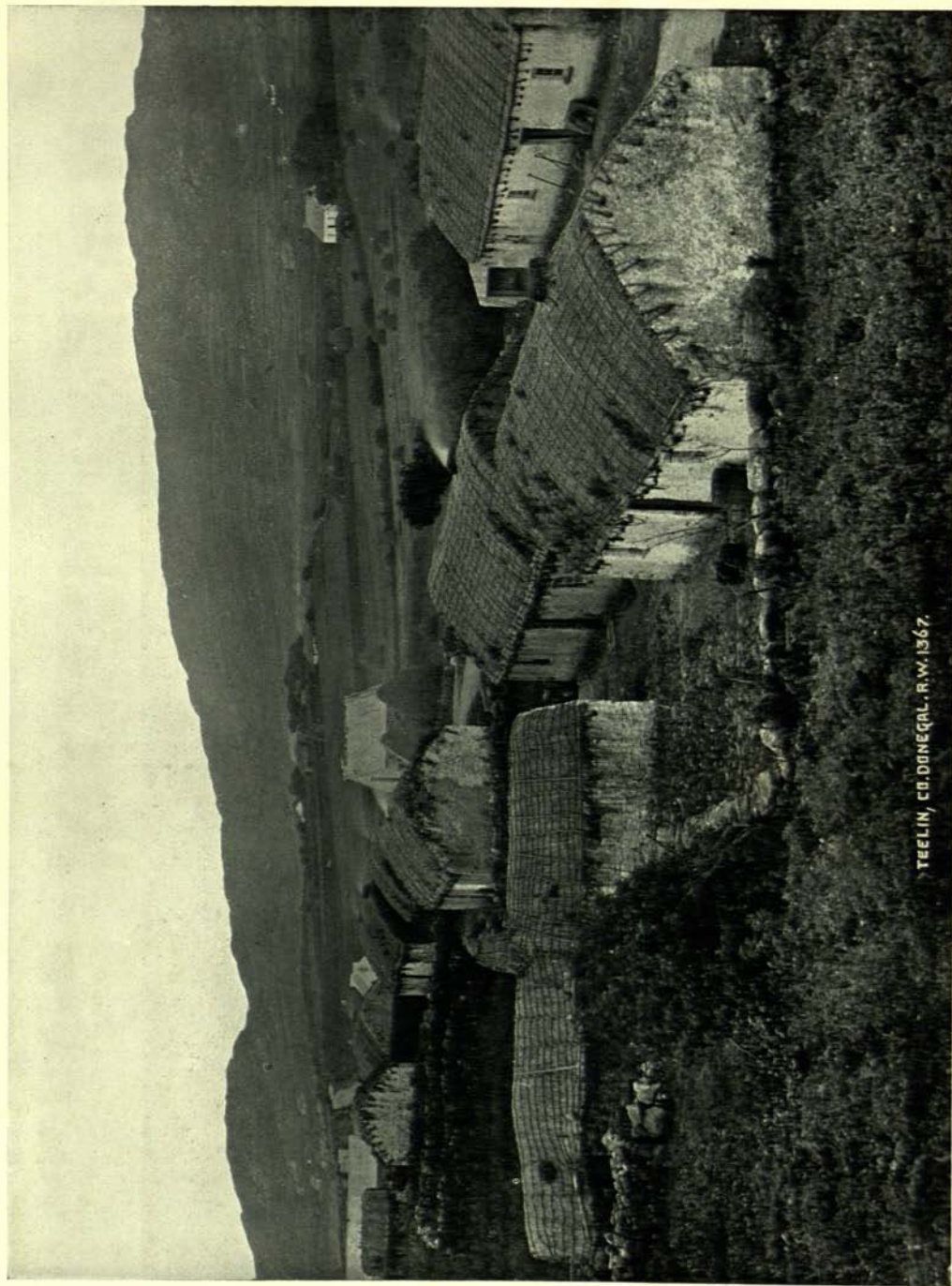
PLATE I



BALLYHOORISKY, FANNAD, CO. DONEGAL. R.W. 2286.

A LOOSE HOUSE CLUSTER—A RELIC OF RUNDLE CULTIVATION, BALLYHOORISKY, FANNAD, CO. DONEGAL (see p. 209)
Ph. R. J. Welch

PLATE II



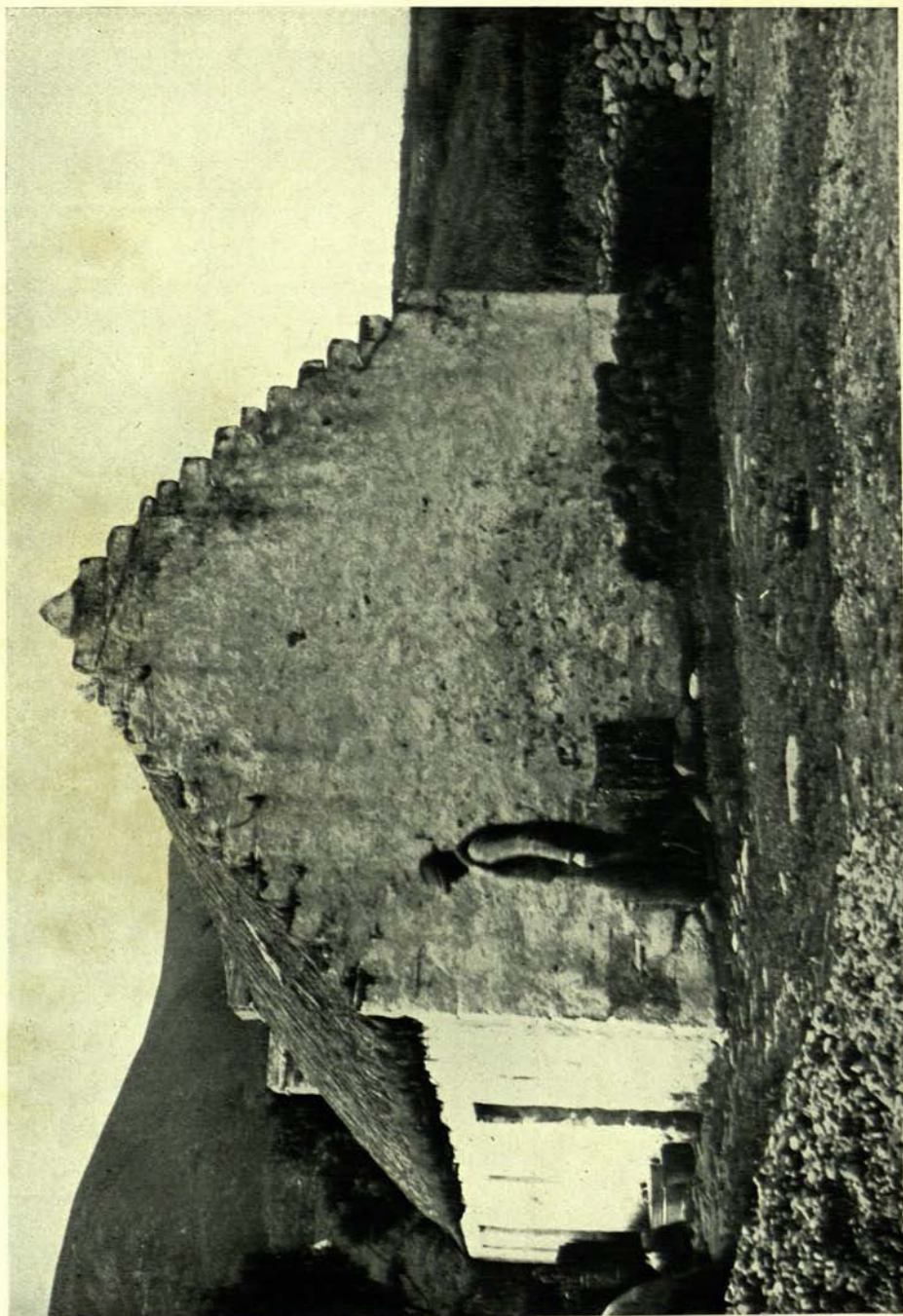
TEELIN, CO. DONEGAL. R.W.1367.

HOUSE-CLUSTER, TEELIN, CO. DONEGAL (see p. 209)

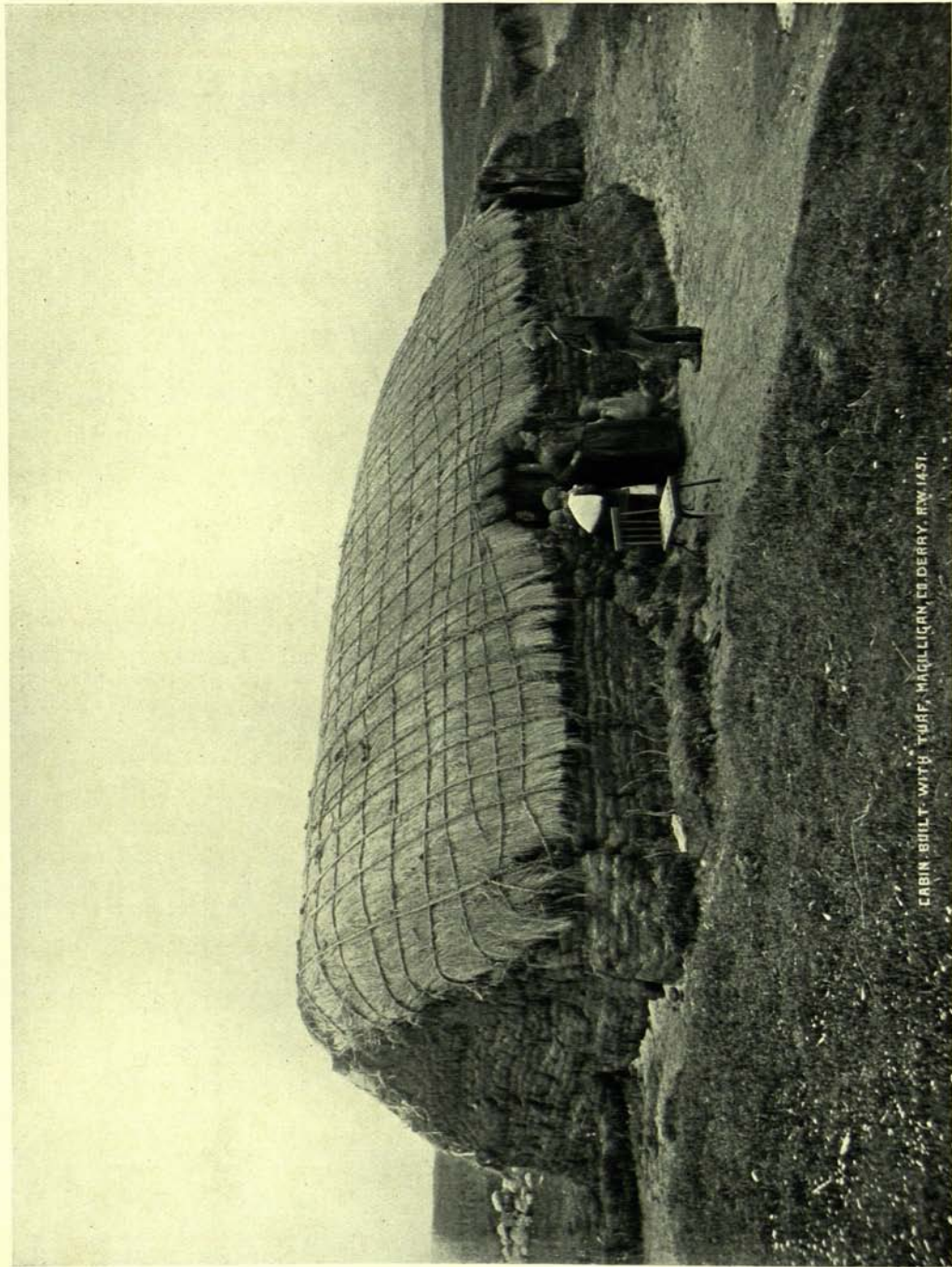
Note the thatched chimneys and the network of ropes with stone pegs at eaves and gables

Ph. R. J. Welch

PLATE III



STEPPED GABLE, MALLARANY, CO. MAYO (see p. 214)
Ph. J. M. Mogey



CABIN BUILT WITH TURF, MAGILLIGAN, LONDONDERRY, IRELAND.

MODERN TURF-CABIN, MAGILLIGAN, LONDONDERRY (see p. 215)

Ph. R. J. Welch

last for a fortnight or so, fresh supplies being brought from the stacks in the haggard. The oats would be thrashed with the flail on the

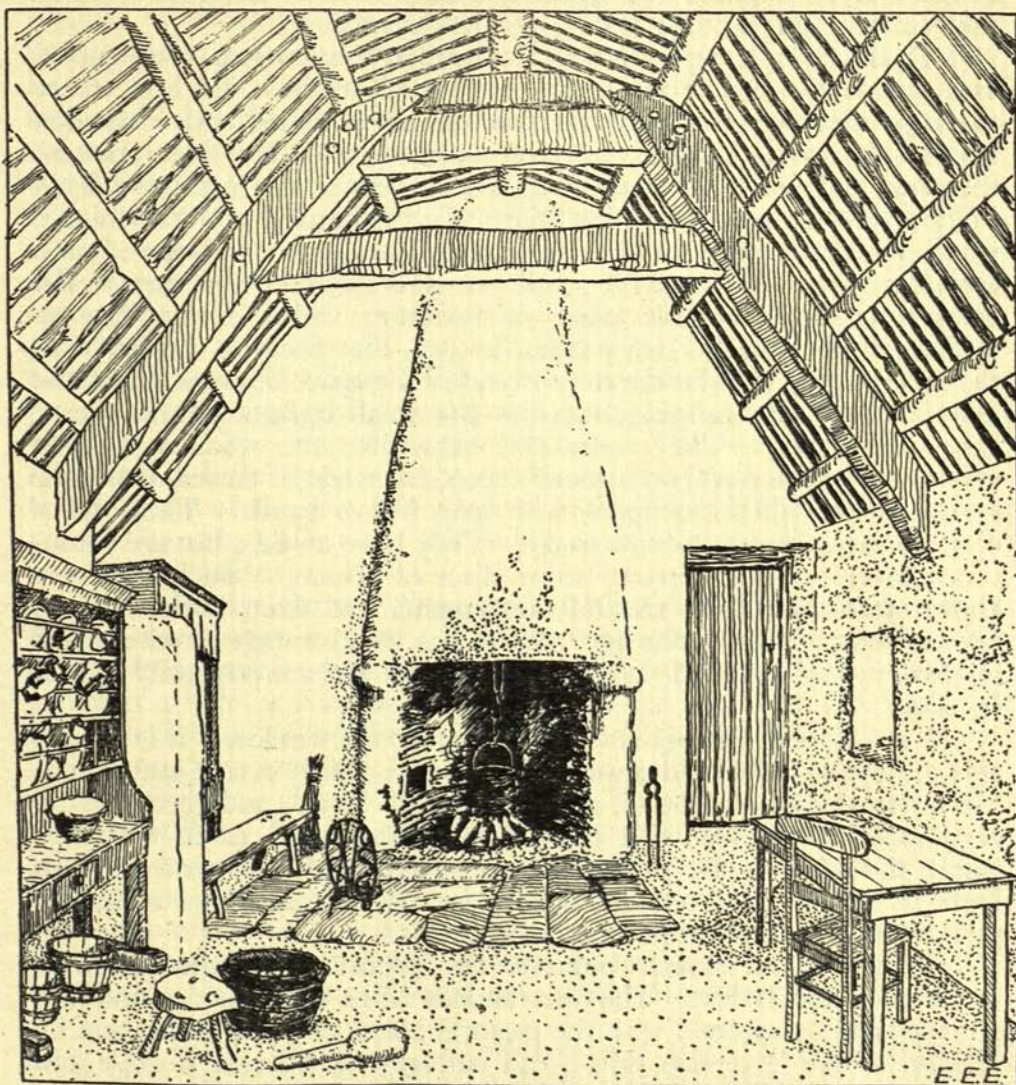


FIG. 6. HEARTH-END OF KITCHEN-BYRE, ERRIGAL, CO. DONEGAL

earthen floor of the kitchen or in the open air, and afterwards kiln-dried and ground in querns. A century ago each settlement had its kiln ;

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they have gone out of use with the decline of the oatcake, nor are querns now seen. The flail is still in general use, and as it is swung to the side of the body it requires little space and may be seen doing its work in the kitchen. An oatcake baking-rest is shown in FIG. 6.

To the right of the front door the earthen floor extends to the paved area around the fire. In it, opposite the small window, is a hole where the large iron pot rests while the potatoes are pounded with a wooden beetle. In paved kitchens a board set flush with the flags answers the same purpose. The wall-bed in its recess is also protected by a small projecting wall 5 ft. 9 ins. high. In the house I am describing a wooden beam resting on this wall crossed the room and served as a roost for hens. Between the small bed-wall and the front door is the dresser, an indispensable piece of furniture which always has its traditional position in Irish peasant houses, that position varying with the house-type. Under the dresser and in the space between it and the door are kept the dairying utensils—the small upright churn, butter tubs and crocks. The churn and tubs, like the wooden milking vessels and piggins still occasionally seen, are made of staves of bog-fir bound, in the older examples, with split willow bands. These relics of a 'forest-culture' deserve study. They have no lids, but are sometimes covered with prepared stone discs of schist.¹⁶ Such discs are known from the Irish sandhill settlements and from burials of the Bronze Age. This 'stone age' device has its advantages; when used for covering milk the lids keep the liquid cool and are not easily moved by cats.

A small table lies against the wall under the window; it is invariably of imported deal and must be regarded as a fairly recent innovation. 'It never attracts the social activities of the house, and there are no ceremonies connected with the places at the table, as in countries where the table is the predominating social and family-centre'.¹⁷ In Donegal there are dwellings where the table is missing, a shallow potato-basket taking its place at mealtime.

Tradition and custom point to the importance of the hearth as the focus of family life. The open hearth helps to make the tradition of hospitality, associated with the pastoral life, a very real thing, and its relation to the 'ovenless thin-bread culture' of Atlantic Europe and to many other elements of rural culture, has been discussed by Erixon

¹⁶ Cf. A. Mitchell, *The Past in the Present* (1880), 60, fig. 39, for examples from Lewis and Shetland.

¹⁷ Campbell, *Folkliv*, *loc. cit.*, 234.

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(‘ West European Connections and Culture Relations ’, *Folkliiv* 1938, II, 137-172). The thin (griddle) bread culture is also linked with the cultivation of oats, ‘ which must be regarded as a specific Atlantic plant ’. This method of baking has assimilated not only wheat and barley but also maize and even the unpromising potato (Irish ‘ slim-bread ’). The Donegal chimneys do not show the built-in stone seats and other developments which in some parts of Ireland incorporate the social tradition in the architecture of the hearth ; they are relatively simple, and there are signs that the chimney is a late addition or accretion. A landlord’s demands for ‘ proper chimneys ’ and ‘ regular funnels for all the fireplaces ’ made about 1850 shows that these conditions were often absent at that time.¹⁸ Where the pot-chain remains in use (the iron ‘ crane ’ is taking its place) it is common for its suspension beam to cross the chimney near the top. Enquiry failed to elicit a satisfactory explanation of this, and I suggest that it perpetuates the pre-chimney period when the pot was hung, as is still the custom in the black houses, from one of the roof timbers. The fire would then have stood out a little way from the gable wall, where it often remains in those houses of the east and south of Ireland which have the fireplace cut off by side walls from the living room (Campbell’s ‘ house with fireplace partition ’).

The turf fire burns at floor level and is never allowed to go out so long as there is turf to burn. The fireplace is floored with small cobble stones from stream-side or seashore. In the side of the chimney recess is a hole or keeping-place in which the man’s pipe is kept. A larger wall-hole behind the bed-head is used for the women’s trinkets. There may be other ‘ keeping-holes ’ in the gable wall and there is one, as we have seen, above the back door. We recall the ‘ ambries ’ discovered at Skara Brae,¹⁹ and may remember further that each hut was found to have its stone ‘ dresser ’. It was probably the coming of the built chimney, together with the provision of separate cow-byres, which reacted to make the back door of the kitchen-byre no longer a necessity. In the absence of a chimney the two doors would be used to control the smoke ; where the half doors were also manipulated for this purpose

¹⁸ Lord George Hill, *loc. cit.* Cf. his description of certain Donegal houses in 1838. ‘ Four walls, built of large rough stones (sometimes they are merely sods), put together without mortar ; no chimney, a front and back door (a contrivance or arrangement for taking advantage of the wind) a small aperture in the wall, to be called, in courtesy, a window, but having no glass in it, a dried sheep skin being its substitute ’.

¹⁹ V. G. Childe, *Skara Brae* (1931).

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the housewife had at her command, as Campbell observes, an elaborate 'draught-machinery'.

Even the humblest dwellings have or have had a bedroom behind the hearth-gable, entered from the living room by a door on the window side of the chimney. Such houses have the chimney near the centre of the roof, but I have stated above that the single room dwelling is the primary unit of construction, and the resemblance to a 'central-chimney' house is superficial. Before leaving the house reference may be made to the custom of keeping three smooth stones on the sill of the window, outside, 'for luck'. I have found prehistoric hammer-stones so employed.

What may be another prehistoric survival is the underground dwelling I was shown, an 'earth house' excavated in a knoll of rotten schist within 250 yards of the house I have described above. It differs in construction from the dry-stone-built souterrains of prehistoric Ireland, but like many of these it has two compartments, pointed out to me by an old man who had slept there when a boy, 'room' (*i.e.* bedroom) and 'kitchen', the latter provided with a smoke-hole leading to the surface. Each room measures some 9 ft. by 6 ft., but the entrance has collapsed. This 'hide' is said to have been made about 150 years ago by the peasants, whose illicit whiskey distillation often brought them pursuers.

Another relic is the corn-drying kiln, formerly indispensable but now very rarely seen in use. They resembled small lime kilns, about 5 ft. high, the bowl-shaped interior having a diameter of 6 ft. at the mouth and 4 ft. at the bottom, and the surrounding wall narrowing from 3 ft. at the base to 2 ft. at the top. The interior was pointed with mortar, and from the bottom a flue 12 inches square led out on the windward side. A two-foot layer of well-dried turf was placed in the kiln and lit with a live turf from the hearth fire. A layer of dry turf mould was added, and when the whole had been glowing for some time the flue was closed and a hurdle or frame of bog oak placed over the mouth of the kiln. On this were placed a thin layer of straw and a linen sheet—coarse linen for household purposes was formerly spun and woven locally—and the oats spread on the sheet and turned from time to time by hand. The dried corn was ground forthwith, the quern being placed on the linen sheet spread on the ground; the oatmeal was stored in a wooden chest.

Curwen considers corn-drying kilns to have been a product of Roman culture in western Europe and Crawford shows that their use

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in Western Britain goes back at least to the 6th century A.D.²⁰ They seem to have been widely used in Ulster down into the 19th century, but I have seen surviving examples only in Donegal and on Rathlin Island.

We have seen that there is no special accommodation for dairy work or even for dairy products in the old houses we have described. If this seems strange in an economy so closely bound up with cattle it should be remembered that in former times much of the milking and butter-making took place in special summer shielings, or booleys, among the hill pastures. In the Errigal district the booleys were described to me as square, some 10 ft. by 10 ft., built of dry stone or sods (see PLATE IV), and measuring from 5 to 7 ft. at the eaves and 8 to 10 ft. at the gables. There were no couples, the purlins, three on each side with a ridge-pole at the top, stretching from gable to gable. Light branches were placed on the purlins and sods and thatch added. Heather was the customary thatching material, and it was secured by ropes of twisted fir fibre, about 20 inches apart, weighted with stones tied to the ends.²¹ The gables of the booley faced east and west; at the west end was the hearth with a hole in the roof above it; but the fire was made outside except in very wet weather. The only door was at the eastern end of one of the side-walls: it had no jambs and was closed at night by a door of woven sally rods or gorse, leaning against the opening.

In many hill-districts in Ireland the foundations of ruined booleys can be traced but until some have been excavated nothing can be said about their construction. It seems likely that stone beehive huts were used in Ireland as in the Hebrides, but this form of architecture survived most clearly in the 'sweat houses' which, though no longer put into service, are fairly common in some counties.²²

Lord George Hill describes a form of transhumance which included movement to adjacent islands:—

²⁰ ANTIQUITY, September 1938, 289.

²¹ This device, still widely employed for holding down haycocks, would seem to be ancestral to the present method of plugging the stones in the wall. An alternative improvement on the 'hanging stones' is found in the practice of letting the stones rest on the thatch just above the eave, as in Connaught.

²² Cf. P. Richardson, 'Sweathouses in County Cavan'. *Ulster Journ. of Archaeology*, January 1939, 32-5. In parts of Mayo and Kerry beehive huts are still used as milk houses and for storing turf. Cf. Campbell, *Folklore* 1938, II, 173-76. In County Louth they are in use as pig-styes.

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'It often happens that a man has three dwellings—one in the mountains, another upon the shore, and the third upon an island, he and his family flitting from one to another of these habitations. This change usually takes place upon a fixed day, the junior branches of the family generally perform the land journey on the top of the household goods, with which the pony may often be seen so loaded, and at the same time so obscured, that little more than the head can be observed; and thus the chair or two, the creels and the iron pot, the piggin . . . creep along the roads. The little churn is slung on one side of the animal, into which the youngest child is often thrust'.²³

Many other ancient customs still survive or have survived into living memory in the Donegal Gaeltacht. Among them we may notice the practices of burning the corn in the ear, and of harrowing by the horse's tail, the use of the wooden plough and wooden-toothed harrows. A host of superstitious practices, notably in connexion with the seasonal festivals of May and November, the beginning and end of the summer half-year, are not yet dead. If an Irish folk-museum on Scandinavian lines is a dream for the future we must be grateful for the brilliant work of the Irish Folklore Commission in rescuing from oblivion the folk culture as well as the legendary lore of the Irish countryside. This scheme clearly requires the cooperation of Northern Ireland, and an attempt is at last being made to organize the collection of material in the six counties. The interpretation of what Mahr has called 'rural sociology' is a task which must ignore political frontiers: fortunately the happy cooperation between Dublin and Belfast in the field of prehistoric archaeology augurs well for the future of related researches into the Irish past.

For assistance in the measuring of houses and general field work I am indebted to several students of Queen's University, Belfast, who spent some time with me in Donegal, and particularly to Miss E. F. Barendt and Mr Angus MacDonald. I am greatly indebted to Mr Patrick Ferry and to Mr Patrick McGuire, both Gaelic speakers, for much information which would otherwise have been inaccessible to me. The photographs by the late R. J. Welch are reproduced by kind permission of the Welch Trustees.

²³ Lord George Hill, *loc. cit.*, 24. Finds of 'bog-butter' hint at an old method of storing dairy-produce.

Avebury

Summary of Excavations, 1937 and 1938

by ALEXANDER KEILLER*

NORTHWEST SECTOR

THE reasons for selecting the northwest sector for the beginning of the excavations within the circles of Avebury, scheduled to last for at least a decade, by the Morven Institute of Archaeological Research (with the permission and cordial co-operation of H.M. Office of Works) was twofold. First, the conditions of indescribable squalor and neglect prevailing over most of the area of this section surpassed, if comparison is possible, those existing in any other part of the circle; indeed the tangle of rusty pig-wire, the accumulations, to a depth of nearly three feet, of old tins and broken bottles, around two of the standing stones, to say nothing of the refuse-heaps which filled part of the ditch almost flush with its edges, contributed ungenerously towards rendering the once majestic site of Avebury what it has been for centuries, the outstanding archaeological disgrace of Britain. Secondly, the presence of a veritable jungle of trees and undergrowth not only perpetually menaced the safety of this part of the monument, but had already done damage to the preservation of the all-important features below ground-level. Protests lodged in the recent past by those who had the welfare of the monument at heart had proved unavailing, and it was not until the eastern part of the sector passed by purchase into the writer's ownership that the work of preservation, which must, at least in the case of Avebury, always be regarded as of so much more importance than that even of excavation itself, could be begun. The scrub and brushwood were removed, as were privet bushes deliberately planted against the stones. The trees, almost without exception rotten to the core and actually growing on the monument, were cut down. The roots in the vicinity of the stones were laboriously grubbed up by hand, while those on the bank and in the ditch were blown up, gelignite being employed in place of any other explosive in order to avoid damage to the subsoil. That the work was undertaken none too soon, indeed in some cases unavoidably too late, was well exemplified when Stone 3, one of the four largest remaining stones in

* See PLATE I, facing p. 232. My thanks are expressed to Major Allen for permitting the use of this air-photograph.

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the Outer Circle, was excavated to its base, and it was found that tree roots had already forced it 1.75 feet out of the perpendicular, while lesser roots had penetrated into cracks in the sarsen and had split large blocks off the parent stone. If the excavations had borne no other result they would at least have confirmed what every archaeologist knows, that the presence of trees is wholly incompatible with the preservation of an ancient monument. Unhappily it may be regarded as a certainty that the northwest sector will not represent, during the excavations at Avebury, the only example of the truth of this dictum.

So far as the northwest sector was concerned no work of an archaeological nature had been recorded, save for a single unproductive cutting into the bank near the northeastern extremity by W. Cunnington and A. C. Smith¹ in 1865. For evidence concerning the number of stones existing at various dates in historical times recourse must be had to the surveys of Smith and Lukis² (1882), Crocker on behalf of Sir Richard Colt Hoare³ (1812), Stukeley⁴ (1724), and Aubrey⁵ (1663).

At the beginning of the excavations four stones* (1, 3, 14 and 15) were still standing, while portions of four more (6, 7, 12 and 13) could be distinguished covered by a medieval field-boundary. This consisted of a low turf-covered dry stone wall connecting many of the stones, whether standing or fallen, and forming, as it transpired, the southern edge of a cart-track running westwards and then southwards from the main Swindon-Avebury road.

The presence of this field-boundary had precluded Smith from carrying out his investigations in 1881, which elsewhere in the Circle had consisted of probing, and in some cases of subsequent partial excavation undertaken in an endeavour to identify the existence both of buried stones and also the position of stone-holes. His survey, however, although showing the same standing stones and the same quantity

¹ Revd. A. C. Smith, *British and Roman Antiquities of the North Wiltshire Downs*, 1885, p. 143.

² *ibid.* p. 140, plate v.

³ Sir Richard Colt Hoare, *History of Ancient Wiltshire*, 1821, vol. II, plate XIII.

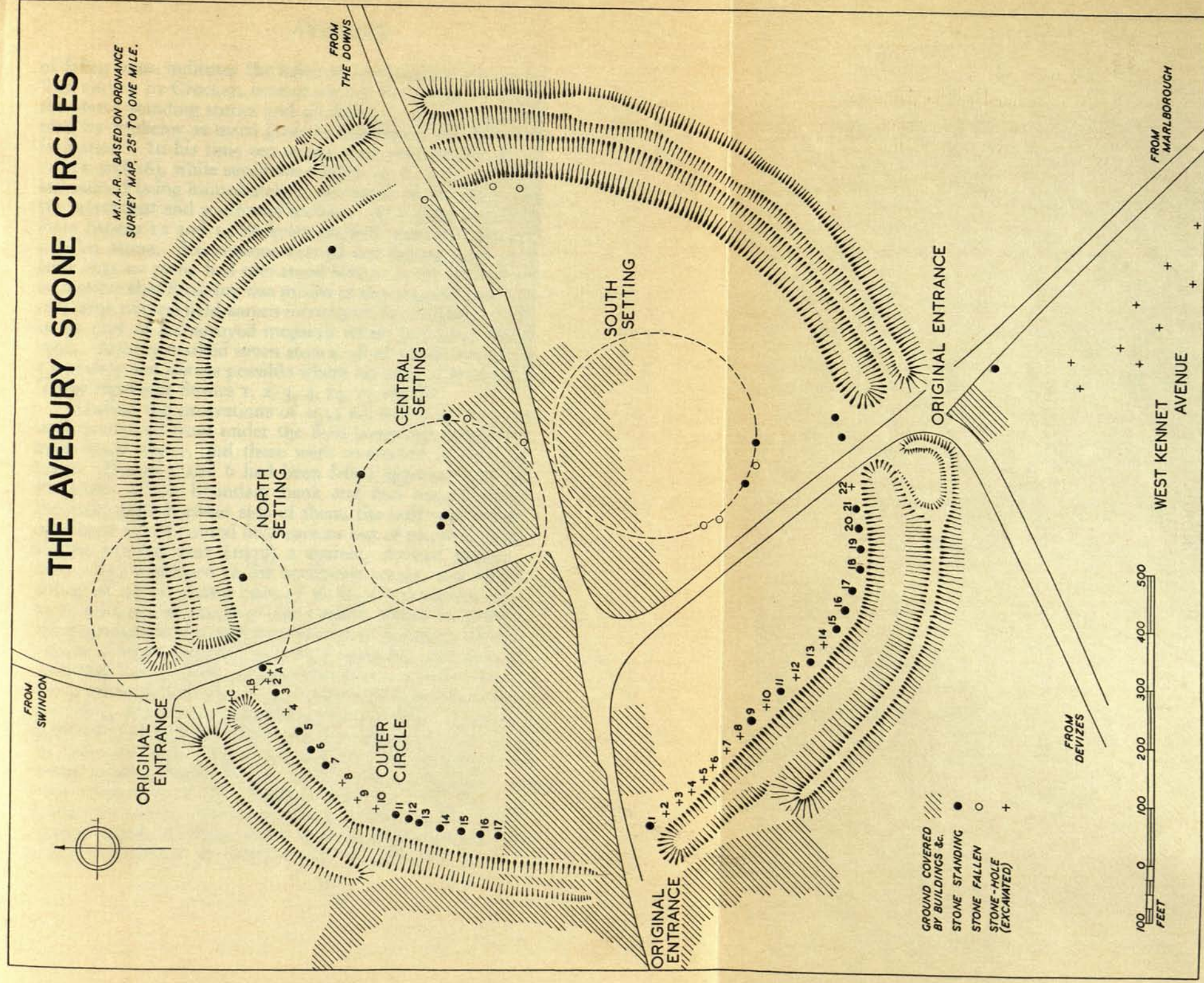
⁴ Dr William Stukeley, *Abury Described*, 1743, Tab. I, frontispiece.

⁵ John Aubrey, *Monumenta Britannica* (Bodleian Library, Oxford); plan made 1663; facsimile of plan in *Wilts. Arch. Mag.*, 1862, VII, 224.

* The system of numeration of stones and stone-holes (or sockets within which stones previously stood) adopted by the Morven Institute in the northwest sector was to allot numbers consecutively in an anti-clockwise direction, Stone no. 1 being the standing megalith immediately to the west of the Swindon-Avebury road.

THE AVEBURY STONE CIRCLES

M.I.A.R., BASED ON ORDNANCE
SURVEY MAP, 25" TO ONE MILE.



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of fallen ones, indicates the latter as representing nos. 5, 6, 11 and 12. The survey by Crocker, carried out for Sir Richard Colt Hoare, shows the above standing stones and six fallen (5, 6, 7, 11, 12 and 13). The plan by Stukeley as usual gives in this regard the fullest historical information. In his time ten stones were standing (1, 2, 3, 4, 8, 9, 10, 14, 15 and 18), while seven were fallen (5, 6, 7, 12, 13, 20 and 21) the last named being immediately to the north of the village street dividing the northwest and southwest sectors. At a point equidistant (96 feet) from Stones 12 and 14 Stukeley marked upon his plan within the area a fallen stone. Excavations carried out during 1937 proved conclusively that no stone had ever stood here or in the immediate vicinity. It is possible that Stukeley was misled in this regard either by the presence of a large natural field-sarsen introduced for building or other purposes, or by part of a destroyed megalith which had been abandoned at this spot. Aubrey marked seven stones, all of which can be identified with a certainty not always possible where his plan of Avebury is concerned. These represent Stones 1, 2, 3, 4, 14, 15 and 18.

During the excavations of 1937 six stones (5, 6, 7, 11, 12 and 13) were recovered from under the field-boundary, to which reference has been made above, and these were re-erected in their original stone-holes. Stones 5 and 6 had been felled apparently at the time of the formation of the boundary bank and had been broken transversely. Between, and on either side of them, the wall was found to have been composed not of natural field-sarsens but of pieces of broken megaliths. In the present year (1939) a system, devised during the previous season and employed in the southwest sector, was utilized here, consisting of taking plaster casts of all fractured surfaces in an endeavour to reconstruct as much of the broken stones as possible. Many of the fragments in the wall were proved to belong to Stones 5 and 6, and certain of these could be, and were replaced; although other portions, including many which joined each other, certainly had formed part of one or other of these stones. In each case at least one course was missing, rendering complete reconstruction impossible. Stones 7, 11 and 12 had been damaged to a greater or less degree in order to clear the route for the cart-track. Stones 16 and 17 were discovered buried in pits of the unusual depth of 8 feet and 7.7 feet respectively. These likewise were re-erected in their original positions. The site of Stone 17 lay within a cow-byre, but on application being made to the owner, Mr J. Peak-Garland, of Manor Farm, he courteously agreed to the demolition of this structure in the interests of the monument. At this stage the

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writer desires to record his sincere gratitude to Mr Peak-Garland, not only in this regard but for his sympathetic interest in the work, no less than for his invaluable practical assistance. This included permission both to remove trees and to re-erect fences, and it is not too much to say that without his continued co-operation the excavations, at any rate of the southern part of the area available in the northwest sector, could not have been carried out. Finally, the stone-holes of Stones 2, 4, 8, 9 and 10, were identified, in each case with an adjacent burning-pit indicating all too clearly the fate which the stones had suffered since Stukeley's day.

In all the stone-holes quantities of clay of riverine origin were found introduced as a packing medium, while a large number of packing stones had been utilized in the stone-holes which had not suffered from subsequent disturbance. Holes which had held small stakes to minimize the friction, which would otherwise have been occasioned by the pressure of the base of the stone upon the side of the stone-hole opposite to that from which it had been erected, were found in the majority of undamaged stone-holes, as well as other features in the chalk indicative of sockets for baulks and vertical posts for various purposes. The stone-hole of Stone 6 was curious in so far that it would appear to have been excavated to a greater depth than was subsequently found necessary, the stone having actually been erected upon a foundation of closely packed supporting stones, carefully laid upright upon their edges. Stones 1, 3, 14 and 15 were excavated to their bases. Stone 1 was found to have slipped in its socket seemingly during, or at any rate shortly after, erection, accounting for its present position, which had previously always puzzled the writer. The position of Stone 14, standing obliquely to the arc of the Circle, has in the past given rise to considerable conjecture, much of it of a fanciful and far-fetched nature. On examination, however, the explanation was found to be quite simple. Owing to the breaking of a horizontal supporting stake, 6 inches in diameter, the stone had imbedded itself up against the northwest corner of the stone-hole, at the same time sliding on the sloping base of the stone towards the north. That this accident had taken place during the original erection was further shown by the fact that the part of the stone-hole in front of the stone, and at the northeast corner, had been filled in with packing stones.

A discovery of considerable importance was made when, on examining material which had been tipped into the ditch to form the course of a medieval track-way, a causeway of solid chalk was found to exist. An original entrance through the bank and over the ditch from the north

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was therefore proved to have existed. The breadth of this causeway could not be ascertained owing to its northeastern edge being at present under the main road to Swindon.

The excavation of a disturbed area of ground a little distance within the Outer Circle and situated between Stone 1 and Stone-hole 2 disclosed the unsuspected existence of a definite stone-hole, to the west-southwest of which, upon a platform slightly below the untouched level of the chalk, lay a quantity of packing stones partly surrounded by a semicircle of closely set stake-holes, which had presumably held anti-friction stakes. So close was this stone-hole (PLATE II, 1), referred to as 'A' on the plan, to that of Stone 1 that it would not have been possible for a stone to have stood in each at the same time, and since Stone 1 has never fallen, Stone A must have antedated it. A further feature of interest lies in the fact that no sign of destruction of this stone by fracture was evident on the site. At a distance of 36 feet to the northwest, during the process of the uncovering of the causeway to the untouched chalk, a second stone-hole ('B' on plan) was subsequently uncovered, although, owing to the scarping of the causeway on both sides to a gradual slope, possibly in historic but more probably in prehistoric times, only the bottom of this stone-hole still existed. A third stone-hole ('C' on plan) was discovered 36 feet to the north-northwest of B (PLATE II, 2). In this case, however, the northeast limit of the ditch had actually cut through the socket, providing thereby conclusive evidence that this stone-hole, and it is to be presumed Stone-holes A and B as well, antedated the bank and ditch and Outer Circle, and that the Circles of Avebury consequently represented a structure of two separate periods. On the assumption that Stone-holes A, B and C formed the arc of a circle, it was shown that a third setting must have existed of practically the same diameter as those of the southern and central circles, while the centres of these three settings would have lain almost in a straight line. It seems impossible therefore not to conclude that what one may term 'Avebury I' consisted of three settings of stones (whether consisting of single circles or double concentric circles remains to be demonstrated by excavation), unaccompanied by banks or ditches. At a later date, although not necessarily after any great lapse of time, a monument of a different type consisting of a ditch, a bank outside it composed of the material from the excavation of the ditch, and a single attendant circle of megaliths on the inner side of the ditch, was constructed, the line of the bank and ditch being broken by entrances consisting of causeways of solid chalk. Whatever

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significance may have been attached to the lay-out of the earlier structure was evidently ignored, if indeed it had not already been forgotten, by the constructors of the second part of the monument, in view of the apparent removal, if not actual destruction, of the northern setting consequent upon the formation of bank, ditch, and accompanying stone-circle. From the evidence obtained during the excavations of the West Kennet Avenue by the Morven Institute in 1934 and 1935,⁶ as well as those of 'The Sanctuary'⁷ on Overton Hill, and of one of the Long Stones at Beckhampton⁸ by Mr and Mrs B. H. Cunnington, it may be assumed that 'Avebury I' is to be allocated to the B1 Beaker-culture during the earliest phase of the Early Bronze Age in North Wiltshire. Mr Piggott has suggested that 'Avebury II' may have been erected by A Beaker-folk at a date which would be contemporary with the later occupation of Windmill Hill. As yet, however, no definite datable evidence to confirm this plausible theory has been obtained. Owing to a certain amount of the bank having been thrown down to form a relatively recent cart-track over the northeast corner, it was necessary to carry out excavations to identify the original limits at this point. During this work sections of a discontinuous palisade trench were uncovered, the purpose of which had been to support the material of the bank in the front and partly round the end. It would appear that no such precautions had been taken beyond this point, nor at the back of the bank where natural silting had been allowed to proceed unchecked. A possible explanation for this feature is that the original erectors were not concerned with the appearance of the rampart unless seen from within the Circle, and the fact that the stones themselves have been dressed only in such a manner as to provide the required forms or shapes, when viewed from a similar position, may be cited as a parallel to this outlook. Alternatively, and more probably, the purpose of those palisades was simply to prevent material silting down from the bank over the causeway or into the ditch.

The entire course of the ditch as far south as the farm road, which represented the southern limits of the area in which either excavation or preservation is at present possible, was cleared of modern and relatively recent refuse to the depth of the top limit of natural silt. This entailed an immense amount of labour, and since no question of stratification was concerned, a mechanical excavator was employed for the

⁶ ANTIQUITY, December 1936, pp. 417-27.

⁷ M. E. Cunnington, *Wilts. Arch. Mag.*, 1931, XLV, 313-14.

⁸ M. E. Cunnington, *Wilts. Arch. Mag.*, 1913, XXXVIII, 3-6.

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purpose. The material obtained therefrom, much of which had been derived originally from that part of the bank which had been thrown down in the 17th century A.D. to permit the construction of the farm buildings which still cover the site, was replaced where the course could be accurately determined. It was found that in historical times not only had material been thrown into the ditch from the western side but the eastern side had likewise been quarried away to a considerable extent from behind Stone 11 to Stone 17. The silted limit of the ditch on the west was identified by the evidence of a buried turf-line, while the original side was shown, after excavation, by the untouched chalk. The original line of the ditch could consequently be restored and this work has been in progress until the present year.

A positive network of medieval and more recent ditches intersected the entire area under excavation. The relative chronology of these has now been satisfactorily arrived at, but this need not be considered as a subject into the details of which it is necessary to enter in this place.

At the conclusion of the excavations no endeavours were spared to improve the condition of this part of the monument prior to throwing it open to the public. Much of the area was turfed, obstructing hedges were removed, boundary fences re-adjusted, or re-erected as far from the Outer Circle as was permissible. Entrance gates were provided at suitable points and finally notice boards containing maps and cognate information were placed at suitable points. As evidence of the increased public interest in Avebury it may be remarked that the attendance of visitors rose, following the excavations under review, from 100/200 a week to an average of between 1000/1500 during the summer months. These figures have not only been maintained but considerably exceeded since 1937.

SOUTHWEST SECTOR

Those who visited Avebury a year or more ago will remember the southwest sector as open pasture, flanked on the north side, except at the northeast corner, by a row of houses in varying states of dilapidation. The field contained one standing stone (no. 17 on plan) and one fallen (11), while two (13 and 18) had been buried in such shallow pits that small parts protruded above the surface. Smith⁹ had referred to the existence of only one other buried stone, which proved on excavation to be no. 9, although situated some distance from the position indicated

⁹ *British and Roman Antiquities*, 1885.

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on Lukis' survey. In Colt Hoare's¹⁰ time no further stones existed, but no. 11 was then standing. Stukeley¹¹ however in addition shows nos. 1, 12 and 14 as fallen, the remains of the first of which were recovered during this season's excavations, while both the others were found to have been destroyed by fire. Nine stones, but whether standing or fallen is not indicated, appear on Aubrey's¹² plan, the identity of some of which cannot however be fixed with certainty.

As an essential preliminary to the work of excavation two derelict cottages, long condemned by the local authorities as unfit for human habitation, were demolished, together with attendant outbuildings and a modern stable. Beneath the foundations of these buildings, and between them and the village street, was found the site of a pond, finally filled in, according to local records, about the beginning of the nineteenth century. In the bed of this, and incorporated with the foundations of an old forge, lay the greater part of a megalith (1) which had been largely broken up by direct fracture. Pieces of this stone were identified in an adjacent buried wall, some of which it was found possible at a later stage to attach once more to the parent block after it had been erected.

To the southeast of the Stone 1, five successive pits (2-6) were uncovered in what had formed the garden of the cottages. In each of these pits stones had been buried; but in every case these had later been rediscovered and destroyed by the fire-and-water method. Had it not been for the subsequent habitation of this part of the monument it is safe to say that all these stones would by now have been re-erected in their stone-holes, parts of which in all cases could be distinguished. Six more stone-holes (7, 8, 10, 12, 14, and 22) were identified with adjacent burning-pits. Besides Stones 9, 13 and 18, referred to above, five more stones were discovered buried, all at a considerable depth. These, together with the fallen stone (11) were re-erected in their original positions.

On the completion of the excavation of the western half of the Outer Circle, therefore, over half the original number of stones, not excluding the area which is still covered by the buildings at the south end of the northwest sector, may be seen standing erect, a much more satisfactory result in itself than could have been foreseen at the beginning of the work. (PLATE III).

¹⁰ *History of Ancient Wiltshire*, 1821.

¹¹ *Abury*, 1743.

¹² *Monumenta Britannica*.

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During the excavation of the buried Stone 16, a complete skeleton was found within the narrow space between the stone and the only unfinished part of the side of the burial-pit. It is evident that the remains were those of an individual who had been accidentally killed while engaged in completing the pit for the burial of the stone, which had apparently slipped or fallen owing to a support giving way, fracturing the victim's pelvis, and also breaking his neck. The right foot was wedged beneath the fallen stone and it had consequently been impossible at the time of death to remove the corpse. It had therefore been covered over and the pit containing stone and body filled in. The date of the occurrence, and the burial of the stone, could be accurately fixed to within a few years by the discovery near the man's left hip of a discoloured patch of soil, doubtless representing the remains of a leather pouch, upon which lay three coins; two silver pennies of Edward I, minted at Canterbury in 1307, and a sterling of the City of Toul. Other finds beside the skeleton included a pair of pointed scissors, which were from their form definitely those rather of a barber than a tailor, and a small iron object, with the vestigial remains of a wooden handle, which had apparently been a lancet or probe. These objects were found beside the left thigh. The discovery of a pair of scissors in England accurately identifiable to so early a date as the first quarter of the fourteenth century A.D. is interesting.

In passing it may be remarked that the generally accepted explanation—that is in order to facilitate agriculture—for the burial of so many of the stones at Avebury cannot, on other grounds than the employment of a barber for the task, be regarded as adequate, at any rate so far as the northwest and southeast sectors are concerned.

The skeleton has been accepted by the Museum of the Royal College of Surgeons, while the associated finds are housed in the Museum of the Morven Institute at Avebury.

The standing Stone no. 17 was excavated to its base, and was proved to have slipped accidentally during erection, much in the same way as had Stone no. 14 of the northwest sector, which accounted for its present position being oblique to the arc of the circle. In this case certain of the anti-friction stakes had been broken by the accident, remains of carbonized wood being found for the first time in the stake-holes.

The total length of the Outer Circle excavated in 1938 was 723 feet, as compared with 567 feet in the northwest sector in 1937.

Owing to modern habitation the Great Ditch was filled almost flush

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to its edges with soil and every imaginable type of filth and refuse from a point opposite stone-hole 6 to the northwestern end. On this being cleared to the depth of the top of the natural silting, and areas of damage to the sides of the ditch repaired, not only was the original outline once more rendered visible, but a causeway of undisturbed chalk, extending under the village street, was disclosed, thus demonstrating the existence of an original entrance on the west, similar to that on the north discovered during the excavations of 1937. Although the connexion of ideas will almost inevitably occur to the reader this is not the place to discuss the increasing probability, for various reasons, of the existence in some form of the 'Beckhampton Avenue' recorded by Dr Stukeley.

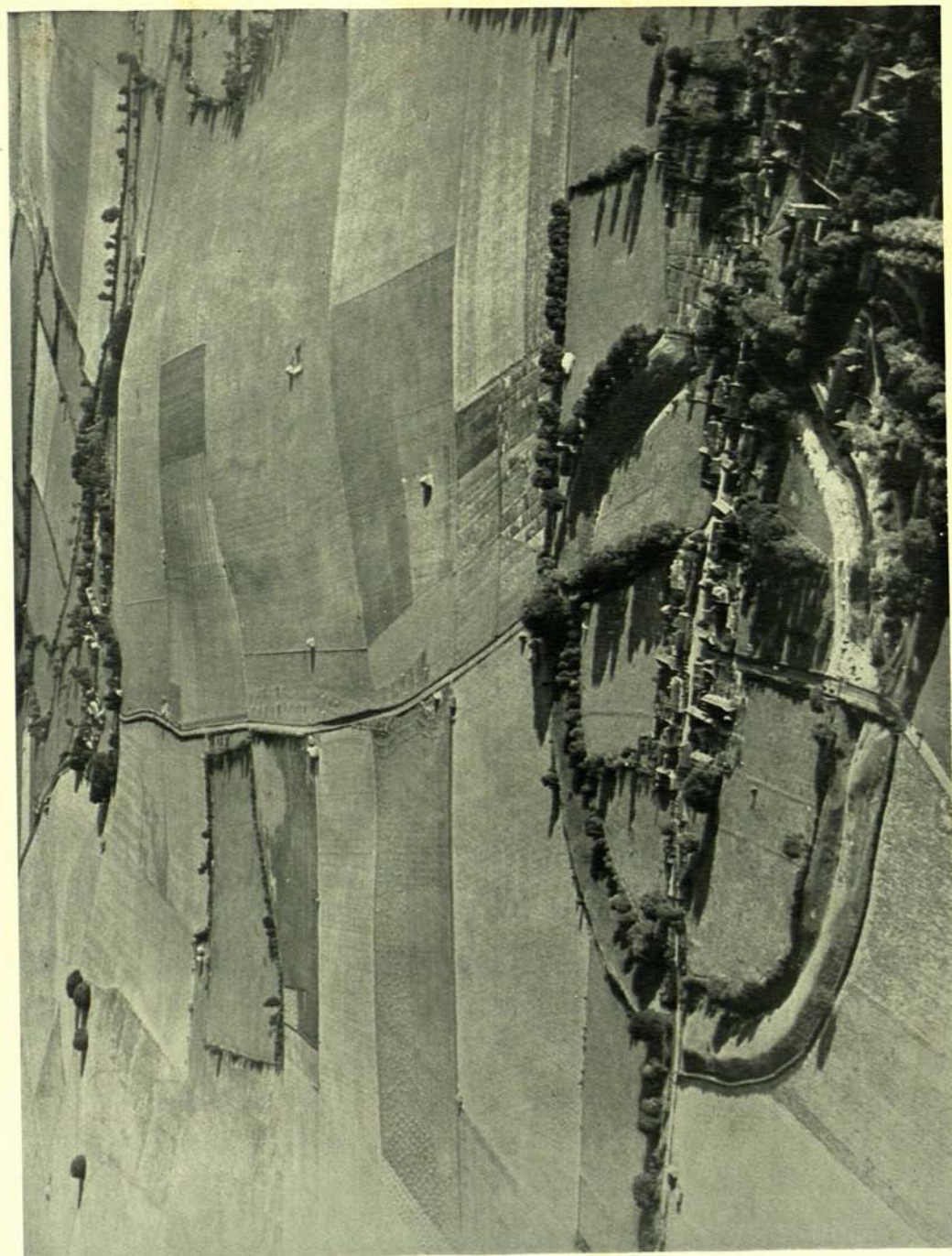
During the excavations of the season under review a cutting was made, behind Stone 11, from the edge of the inner side of the ditch down to the level of the natural silt. A similar cutting, opposite to this, was undertaken on the outer side of the ditch, and finally a section, 10 feet broad, was excavated horizontally into the bank itself. This last disclosed features of considerable interest. First, upon the clearly marked original turf-line, was exposed a face of dry walling, three feet high, and five feet thick, composed exclusively of blocks of Lower Chalk, which had, owing to the seeping of water through the thin turf covering, by now taken on an almost circular shape with a curious form of laminated decortication, more reminiscent of large snow-balls in a thaw than anything else. Behind this retaining wall the main body of the bank was seen to have been thrown up in a series of heaps or cones, consisting of the material obtained from the excavation of the ditch. These heaps had, during construction, silted down towards each other to a certain extent, and the intervening spaces had been further filled up with fine rubble. The uneven appearance of the top of the bank (except in the northwest sector where the bank has been artificially flattened, probably during the planting of trees), so familiar to visitors to Avebury, may thus in great part be explained as representing an original feature. (PLATE IV).

The only object found within the bank was a piece of a worked rib of ox similar to those recorded by Mr Gray¹³ during his excavations in the ditch as well as in the bank at Avebury between 1908 and 1922, and the two specimens, reported by Mrs Cunnington,¹⁴ from the bottom of

¹³ H. St. George Gray, *The Avebury Excavations, 1908-22*, *Archaeologia*, LXXXIV, 1934, pp. 121, 127, 131, plate XLVIII.

¹⁴ M. E. Cunnington, *Woodhenge*, 1929, pp. 110, 111, plate 21.

PLATE I



AVEBURY, WITH EXCAVATIONS IN NORTHWEST SECTOR IN PROGRESS. THE WEST KENNET AVENUE IS SEEN IN THE MIDDLE DISTANCE
Ph. Major G. W. G. Allen

PLATE II

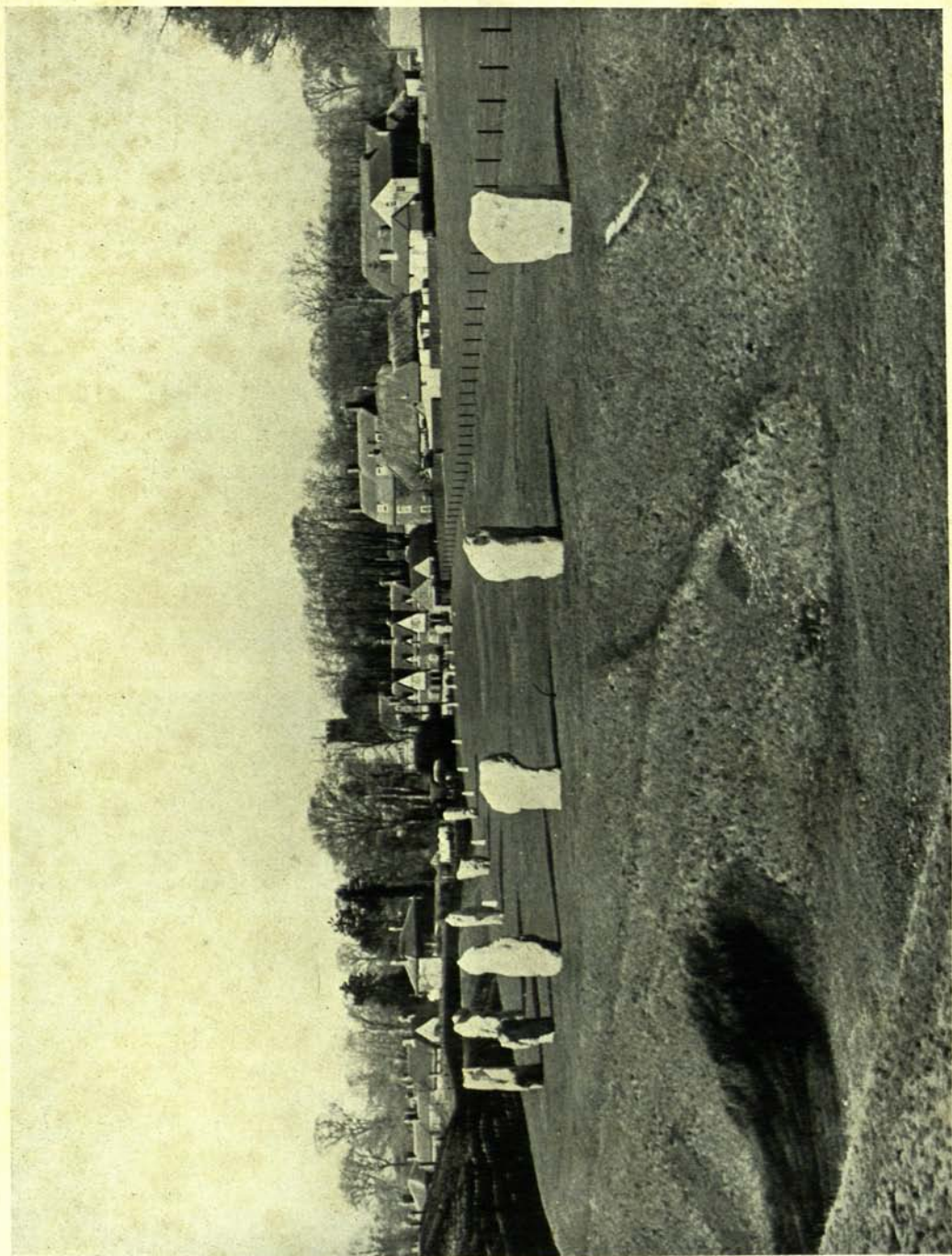


AVEBURY : NORTHWEST SECTOR—STONE-HOLE A OF NORTHERN SETTING (*see p. 227*)

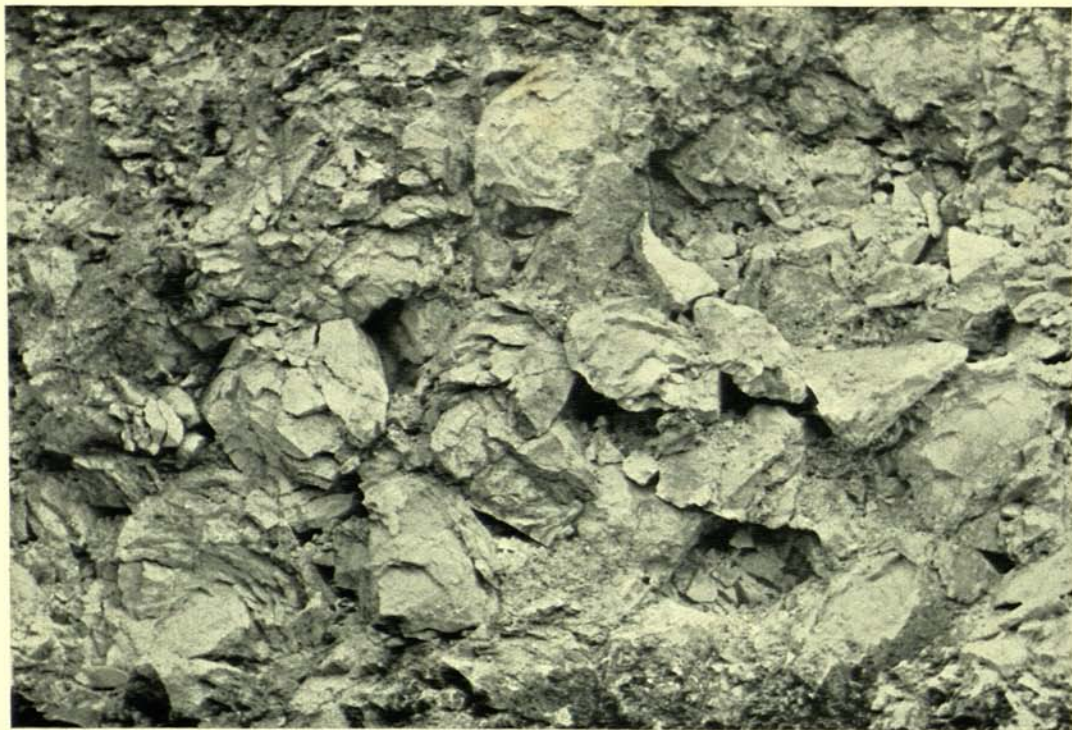


AVEBURY : NORTHWEST SECTOR—STONE NO. 1, AND SITES OF STONE-HOLES A, B, AND C (*see p. 227*)

PLATE III



AVEBURY: SOUTHWEST SECTOR AFTER EXCAVATION, LOOKING NORTHWEST (see p. 229)



AVEBURY, SOUTHWEST SECTOR : RETAINING CHALK WALL OF BANK (*see* p. 232)



AVEBURY, SOUTHWEST SECTOR : SECTION OF BANK BEHIND RETAINING WALL

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the ditch at Woodhenge. No satisfactory suggestion has been put forward regarding the purpose of these articles, but their occurrence in each case in association with digging operations in chalk cannot be overlooked.

Sealed beneath the original turf-line were found much-abraded sherds of Neolithic A pottery in the vicinity of two stake-holes and a formless, though artificial, depression in the chalk, presumably of the same period, and in any case antedating the construction of the megalithic monuments of Avebury.

On June 1st 1938 the Museum of the Morven Institute, where exhibits in connexion with these excavations are displayed, was officially opened to the public, over six thousand persons visiting the building within the following five months.

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BRONZE STATUE FROM KHUZISTAN, IRAN (PLATE 1)

In the *Geographical Journal* (pp. 324-6) for October 1938, is published a photograph of a fine bronze statue (plate 10, opp. p. 324) discovered by Sir Aurel Stein during his recent 'journey' (as he modestly calls it) in western Iran. By courtesy of Sir Aurel and of the Royal Geographical Society we are able to reproduce the photograph (PLATE 1) together with his account of it. He asks us to state that a full account of the site and of other finds brought to light there by his excavations will be published in his detailed report.

The statue was encountered in the house of the military administrator of the Malamir district. It is carefully cast, 6 feet 4 inches high, wearing on the head a diadem-like band, and has a sword and dagger suspended from a waist-belt. 'While the dress of the figure was Iranian, as also the arrangement of the hair, the excellently modelled head showed unmistakably the influence of Hellenistic art. The features of the face curiously recalled those represented in certain Graeco-Buddhist sculptures of Gandhara. From the same find came fragments of smaller bronze figures and two marble heads. One wears the royal diadem and the other, badly damaged, is that of a Hellenistic type of Aphrodite, like that found by me in 1934 at Fasa. All these sculptural remains could clearly be recognized as dating from Parthian times, the least known period in the history of Iranian art'.

The statue was found six months before Sir Aurel's visit in the valley of Shami, and the site is not far to the west of the intersection of Lat. 32° N. and Long. 50° E. He proceeded to the spot and the rest is best described in his own words (G.J. pp. 325-6):—

'Retracing from Susan part of our route and then crossing a low pass to the north, we reached Shami by January 27. There in the steep but fairly open valley known by that name we were shown the spot where the bronzes had been discovered. It lay at an elevation of about 3600 feet on one of the terraces which stretch down from a rugged high mountain on the west and offer some ground for cultivation. A short while ago an order had gone forth here to the scattered camps of still semi-nomadic Bakhtiari that all claiming arable land should settle down near it in permanent habitations. So one of the Shami people had started to build himself a dwelling with the rough stones

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plentifully lying about on that terrace. In digging down for a foundation wall he had struck the bronze statue seen at Malamir at a depth of a couple of feet. After a day's digging done in the presence of the district officer had brought to light the fragments of sculptures also removed to Malamir, the place of the find had been left undisturbed under administrative orders.

'A week of steady excavation now allowed me systematically to clear the whole of the low mound from the southwestern corner of which the sculptural remains had been recovered. It proved to mark the site of a quadrangular temple, measuring about 76 by 40 feet, which had been destroyed by fire and sacked. Subsequently its walls had been levelled to the ground to provide space for some later structure. But the foundations of the walls could still be traced, and in the centre of the quadrangle an altar built with burnt bricks and approached over a brick pavement had survived.

'There was only too clear evidence that this site, dating from Parthian times, had after its first destruction been greatly disturbed by quarrying operations for metal and other useful materials. This was proved by the disturbed position of six massive stone bases which had once carried statues. On some of the bases there were sunk sockets for the feet of more than one figure. Among the numerous broken pieces from bronze images, all cast and clearly showing in their modelling the influence of Hellenistic art, there were several which had belonged to statues over life-size.

'The most striking of these remains are pieces of a fine male head, obviously smashed with violence. The two halves of the well-executed mask can fortunately be fitted together and show a face in a distinctly naturalistic treatment of Hellenistic style [plate 9 in the original account]. A third piece found close by shows the back perhaps belonging to the same head. The diadem tied over the locks indicates that a royal personage is represented. Worship of kings deified is well attested among Alexander's successors in the Near East, and expert examination of the head, when the portions have been properly joined up and cleaned, may yet lead to the identification of the king to whom the image was dedicated.

'Among the fragments recovered from other bronze statues I may mention a carefully modelled colossal hand which, by the pose of the fingers and wrist, suggests that of a divinity holding a sceptre or spear. Besides other sculptural fragments, we recovered a miniature altar of marble, delicately carved; pieces worked in repoussé of two ornamental

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candelabras of copper, perhaps for burning incense ; the elegantly designed bronze foot of a chair or table representing the foot of a lion ; and a sheathed steel dagger. A small copper coin from the mint of some Greek town in Mesopotamia afforded a welcome indication of the approximate date when the shrine was still visited.

There remains the puzzling question as to how a locality, so restricted in space and resources as this outlying valley of Shami is, could afford a temple so amply provided with objects of Hellenistic-Iranian worship. It is hard to assume that objects of such size and weight as a colossal bronze statue could have been brought from any great distance to a place surrounded by rugged mountains. On the other hand, if it was cast here, what highly developed cultural and economic conditions it presupposes for such a locality. A thorough exploration of certain ruins just traceable below the surface of cultivated terraces lower down might perhaps yet help to answer such questions. But even without this there is reason to feel grateful for the light thrown by the finds of Shami on a fascinating but as yet very scantily documented phase of art in Iran'.

EXPEDITION TO SWAT AND AFGHANISTAN

For the last twenty-five years it has not been possible to add to our knowledge of the Art of Central Asia by further exploration of the sites on the trade routes skirting the Tarim Basin. But since the last expeditions of Stein and Le Coq to these regions, the French Delegation's discoveries in southern Afghanistan have made desirable a fresh approach to the chronology of the whole complex of Buddhist Art from northwest India to Central Asia. While political difficulties still make it impossible to work in Sinkiang itself, an attempt was made in the summer of 1938 to plot a few points in the earlier stages of the culture route, which seems to lead from northwest India over the Hindu Kush and northeast through Badakshan on its way through the Central Asian desert to China.

A lightly equipped expedition of four, which left England at the end of May, under the auspices of the Victoria and Albert Museum and the R.G.S., divided its time and forces between an archaeological reconnaissance of northern Afghanistan, some parts of which have not been travelled by any Englishman for a hundred years, and the survey and excavation of Greco-Buddhist sites in the Swat Valley, beyond the northern edge of the Peshawar Plain. In Swat, Sir Aurel Stein had visited and surveyed some of the sites in 1926, but until 1938 no

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archaeologist had been permitted to excavate. About three months were devoted to the excavation of some of the numerous monastery sites in this once populous centre of Buddhist civilization, and it was possible to make a representative and well documented collection of sculptures. By mapping all the remains of the Buddhist period—monasteries, shrines, forts, villages and the cultivation-terraces which are still a prominent feature of the Swat landscape—an attempt was made to reconstruct the appearance of the country during the first five or six centuries A.D. At a mound near Charbagh, in Upper Swat, excavation revealed walls of the Buddhist period, pottery, terracotta figures, beads, ironwork and other evidence of ancient occupation.

In Afghanistan the French Delegation has held a monopoly for archaeological work since 1922. It was largely owing to its generous cooperation that two members of the expedition were able to make an archaeological tour, though not of course to excavate, in northern Afghanistan. Their object was to find out what surface-ruins exist; what sites might be worth excavation in future, and what evidence of occupation in ancient times had been found locally. Greco-Bactrian coins and Greek and Sassanian seals were acquired at various places, but the most important discovery was that of some Hellenistic column bases at Kunduz. This find emphatically disproved the current theory, largely based on M. Foucher's fruitless excavations at Balkh, that the buildings of ancient Bactria were made entirely of mud. Near the scene of this discovery there is a gigantic 'castle', half a mile in circumference with walls of mud a hundred feet high. This is probably a Sassanian stronghold; the pottery, which owing to the excavations of local quarriers could be collected from the lower levels, included Sassanid examples. After a brief tour of Badakshan, during which the members of the expedition located the probable remains of the ancient capital of the province on the southern branch of the silk route, they visited Balkh, where the surface remains are a confusing wilderness of mud walls of uncertain age. In the plain west of Balkh they traced a number of systems of ancient irrigation canals and examined various groups of mounds. Here again pottery fragments, mostly of the early Islamic slip-painted ware type which they collected, should give some clue as to which types of mound are ancient and worth excavating.

The finds of the expedition are at present exhibited at the India Museum, South Kensington, for the next few months.

EVERT BARGER and PHILIP WRIGHT.

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DISCOVERIES AT MERSIN (PLATE II)

We are indebted to the Editor of *THE TIMES* and to Professor John Garstang for permission to print the article by the latter on Mersin, Cilicia, in *THE TIMES* of 10 March 1939, page 17, and for permission to reproduce the illustration (PLATE II).

The discovery of well-preserved remains of a fortified city, with developed features of civil and military architecture, 20 ft. below the imperial Hittite fortress uncovered last year, marks the climax to another instructive season's work. Happily the evidence on the relative dates of these finds is both clear and plentiful; but as it is technical in character a few lines must suffice here. The early architectural remains were found well down in our chalcolithic deposits, below a stratified series of building levels in which the culture was related to that of Uruk and El Ubaid. They also contained internal evidence of direct contact with the polychrome phase of the still older Mesopotamian culture known by the name of Tell Halaf. These indications fix the position of our finds in relation to the pre-dynastic civilizations of Mesopotamia; and an approximation in date to 3600 B.C. does not exaggerate their antiquity. Hitherto architectural remains of this remote age have been limited to a few incomplete houses and rooms on North Mesopotamian sites; yet in the westernmost corner of the Cilician plain, 150 miles from the nearest known Halafian centre, we find a whole group of good buildings laid out with plan and purpose.

Some of their architectural details are indeed astonishing. Though mud bricks were used for building in preference to undressed stone, they were large and regular in shape and size. Alignment was good, even the corners of rooms were reasonably square, and the faces of walls were plastered. Room walls were about 2 ft. thick, but main house walls might be twice that size. The outer defensive wall was, in fact, 5 ft. in thickness, and standing on an outer terraced foundation of stone it is preserved to a height of 7 ft. or 8 ft. It is pierced by a continuous range of narrow apertures, 8 in. by 20 in., which traverse with parallel sides the full thickness of the wall. On the inside a row of uniform rooms abuts against the main wall, and in these the apertures appear like windows, regularly spaced, two to each room. They seem low, but they are placed at a convenient height for kneeling archers using bows about 4 ft. long. The minimum span of the rooms was about 13 ft.; for their roofing supplies of good timber were available

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on the lower slopes of Taurus, a factor which helps to explain no doubt the unparalleled architectural developments of the site.

The continuous roof of the rooms would form a defensive platform, and supplies of home-made missiles suggest free use of the sling. The fact that these missiles were made of clay, albeit shaped and partly fired, and that they outnumbered the stock of selected river stones piled with them, suggests that the defenders of the walls had to withstand a siege and were driven to this expedient to supplement their ammunition. In the end the defence was apparently overwhelmed, the city was taken by assault, and the complete contents of its rooms lay buried beneath the débris of fallen walls and burning roof timbers. Though much damaged, the furniture of the rooms is found to have been more or less alike, including a grain bin, grindstone and pounder, fireplace, and cooking pots, as well as a number of storage jars and smaller vessels decorated with painted designs. Each room, moreover, was fronted by an enclosed court, around which a series of post-holes indicate a verandah or roofing of light materials. Thus these rooms against the main wall, though dedicated to the defensive necessities of the town, constituted small domestic units; they formed in all probability the married soldiers' quarters.

Turns in the main wall were covered by stout offsets, each sector of the masonry continuing straight; but at its western extremity, overlooking the river, where the angle would have been sharp, a main gateway intervened. This was protected by an extra-mural tower and a small internal guard-room. The protected passage was about 12 ft. long, the width of the doorway nearly 6 ft. Just within, by the side of a mural recess, was a stout mounting-block, the upper side of which was worn smooth with long use. The main wall on the west side was found to be denuded by erosion of the river, but in this quarter of the city there had stood a much larger detached building of domestic character, presumably the residence of a chief. Its central feature was an open court, 36 ft. long and 12 ft. broad, surrounded by good strong walls. Within this enclosure was a large baking oven of familiar oriental character. On either side was a row of well-built rooms about 15 ft. square, only one of which was rather narrower, more like a passage, and gave access by an outer door to an open space within the city, just opposite the watergate. The house formed a square block of rooms on either side of a central court.

It is already obvious that we have found in Cilicia a centre of civilized development which was already well advanced when Egypt, Babylonia, Crete and Europe were still in infancy. Architectural indications

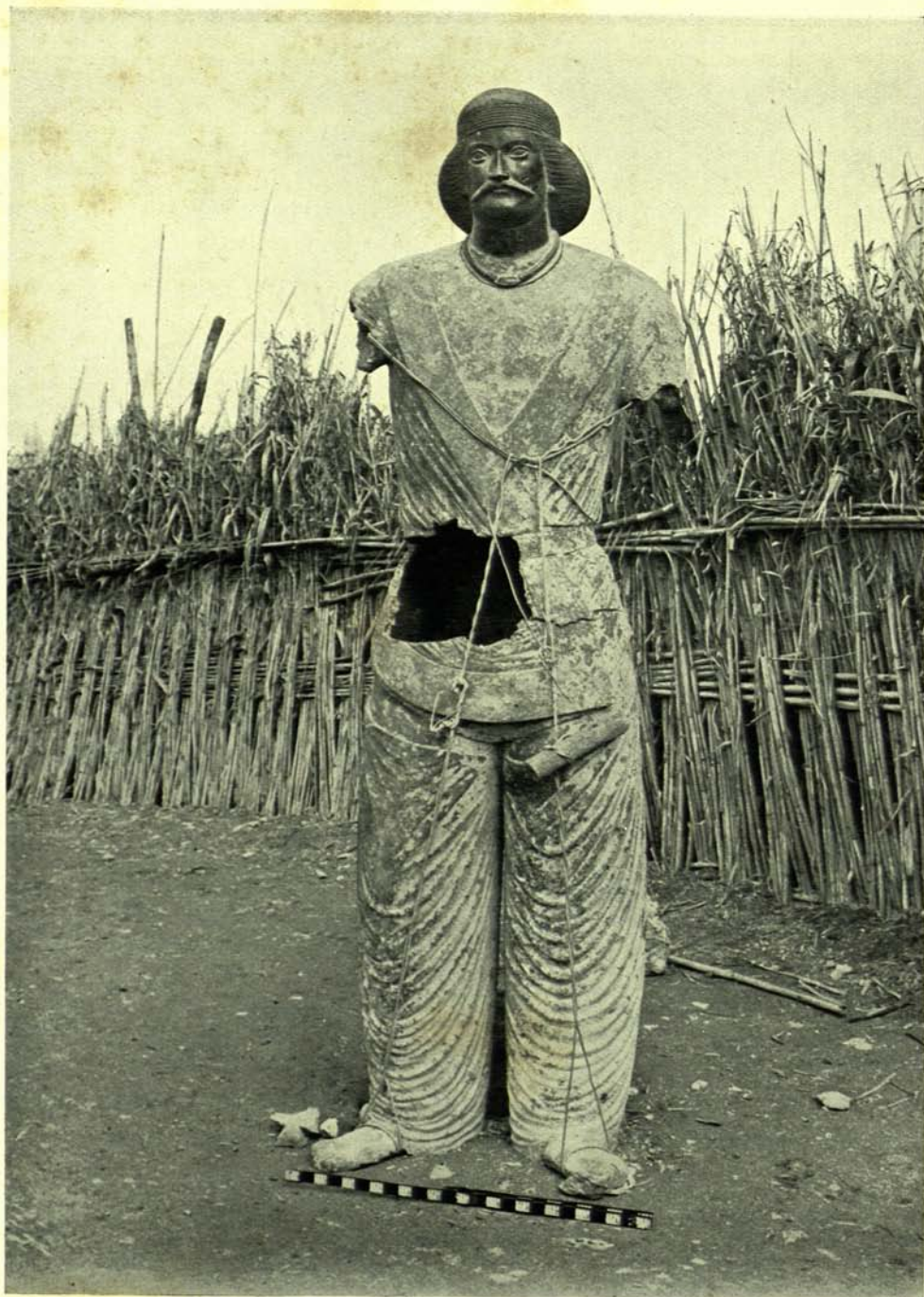
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are confirmed in other ways. The painted pottery of Mersin is a distinctive feature of many successive ages ; but at this period, though entirely made by hand, it had already attained its zenith in variety of form and decoration and details of technique. The finest examples of these wares were found in the building last described. Even the kitchen pots, though plain, were freely burnished, the most attractive being finished with a shiny black or deep brown surface and furnished with elaborate handles of a distinctive kind. The common variety of local painted jar was based in shape on a gourd with shortened neck, and was decorated with linear patterns done in matt black paint upon a burnished cream surface. Less common but even more attractive were a number of trichrome jars of varying shapes, on some of which the rosette appears as the central motive of the decorative scheme. This was carried out, in accordance with local tradition, in matt black upon the reserved cream surface of the pot ; while the body of the vessel was further treated with broad bands of red or salmon-pink paint bordered with deep lines of black. In the same building was found an imported fragment of polychrome ware in the later Halafian style, while copper axes, flint sickle blades, and numerous obsidian chippings confirmed the chalcolithic character of the deposits at this level as a whole.

Such in rapid outline were the chief features of our 16th level. The buildings, which represent the oldest architecture yet known, have been left standing for all to see until the work is resumed next autumn. The clarity of the ground plan and details is a tribute to the skill and devotion with which Mr Seton Lloyd carried out his duties as field supervisor. It is indeed one of the clearest pieces of excavation I have ever seen, and in this respect Mr Seton Lloyd's long experience of Mesopotamian sites has proved of great value to the expedition. If in describing the features of this level I have stressed the oriental links it is for the simple reason that these enable us to ascribe approximate dates to the remains. But certain suggested contacts with the West are even more fascinating, for they lead us into areas of prehistoric Europe, in particular the Danube basin, where hitherto no direct dating links have been established.

Our 15th level (PLATE II), which overlay that last described, was distinguished architecturally by a massive gate-tower 25 ft. square. The cultural affinities at this time were with El Ubaid rather than Tell Halaf, but among the local wares the trichrome style of decoration persisted, though the rosette had disappeared and the designs were much simplified. Large curving bands of salmon-pink between broad black

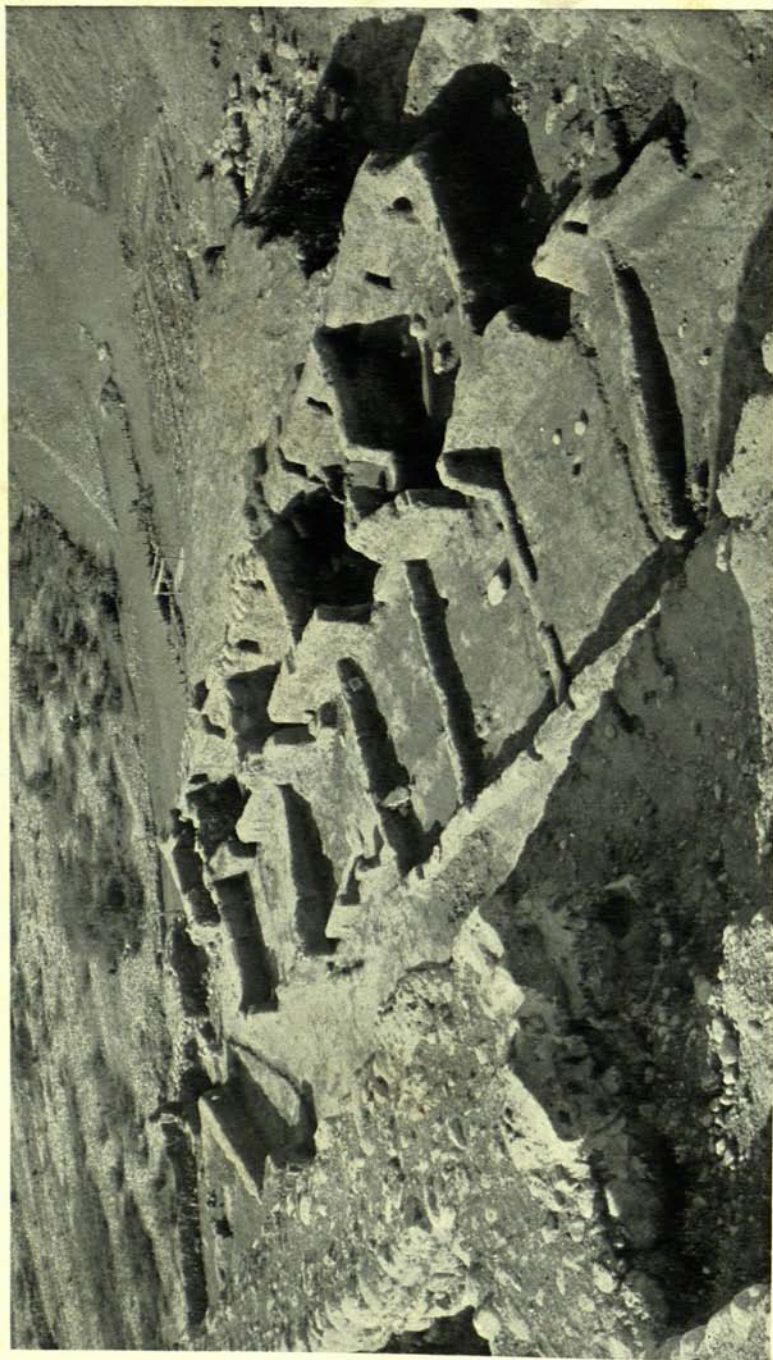
PLATE I



BRONZE STATUE FOUND BY SIR AUREL STEIN IN WESTERN IRAN (see p. 234)

By courtesy of Sir Aurel Stein and the Royal Geographical Society

PLATE II



MERSIN, CILICIA

Buildings at Level XVI, showing rooms against the main wall and apertures in the latter, 3600 B.C. At the far end of the row is the gateway and mounting block. In the foreground (left) is a corner of the Hittite Fortress at Level VII, 1300 B.C. (see p. 238)

By courtesy of Professor J. Garstang

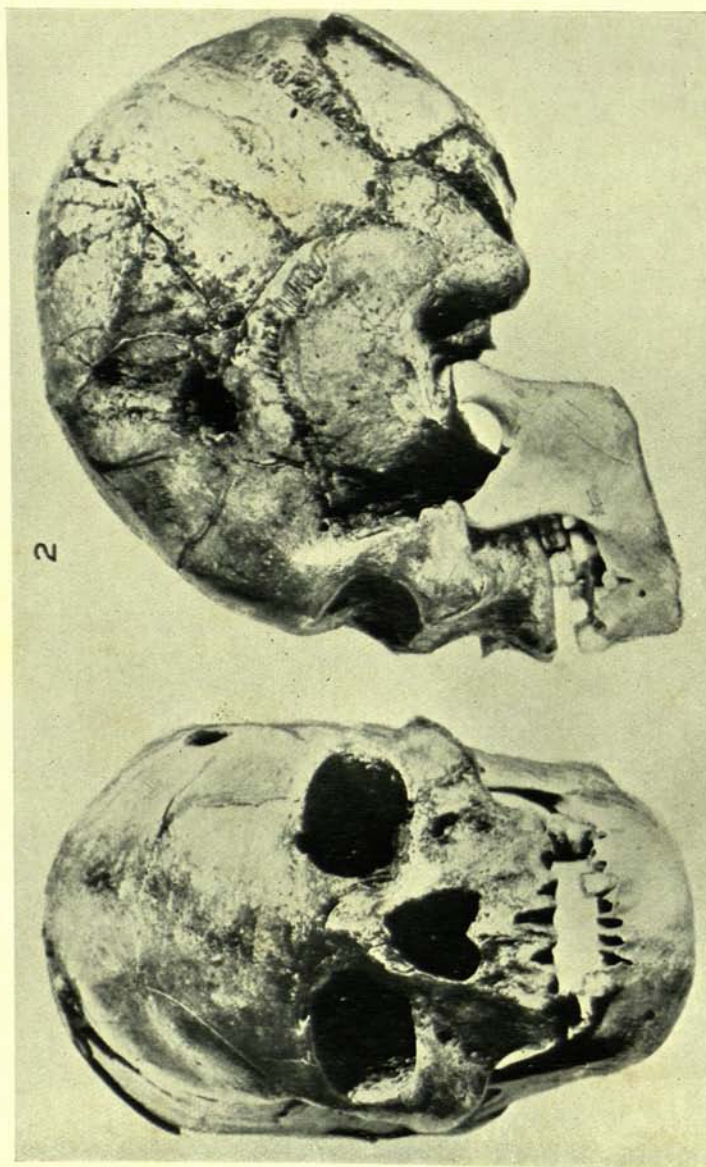
PLATE III



HOMO SAPIENS AT CHOUKOUTIEN, CHINA

Work in progress near the Upper Cave at Choukoutien. The first remains of an Upper Palaeolithic settlement found in Asia, except in Palestine (see p. 242)
By courtesy of 'The Times'

PLATE IV



HOMO SAPIENS AT CHOUKOUTIEN, CHINA

The 'Melanesian' Woman's skull. It is clear that a sharp instrument has penetrated the left side of the skull, and the depression at the top of the forehead can be seen (*see p. 243*)

By courtesy of 'The Times'

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lines were laid upon the cream surface of the pottery, and decorative details were adapted to the varying shapes and sizes of the vessels. Similar tricolour schemes applied to curvilinear designs are a special feature of certain painted neolithic wares from Bessarabia, in the Carpathian area of the Danube basin. If comparison confirms the superficial resemblances prehistorians of Europe will find fresh material for that chronological revision towards which some of our leading experts are already feeling their way.

But similar trichrome treatment is not uncommon, as witness the ancient wares of Jemdat Nasr and more recent products of the Algerian Kabyles, so that the suggested contacts with prehistoric Europe, if entirely unsupported, would not be worthy of mention at this stage; but there seems to be further evidence of like kind in our deeper levels. Nor should we overlook in this connexion the striking series of chalice forms and bowls decorated with flowing white lines upon a burnished black or deep brown surface. This new type comes from our uppermost chalcolithic level (no. 12), in which Uruk grey wares were also present; and the ramifications of its relationships may carry us even further into the prehistory of the West.

Though I have referred to the uncovering of our sixteenth level as marking the climax to the season's work the anti-climax is not less astonishing. It should be borne in mind that our mound rises 80 ft. above the water level—*i.e.*, about 70 ft. above the level of the plain. Now our sixteenth level, which we have dated to 3600 B.C., was found at a depth of some 35 ft. below the summit of the mound, less than half-way down. But our soundings show that the whole of the underlying strata represents human occupation. We have followed down a further 10 ft. the traces of an earlier chalcolithic culture; and below that again, for no less than 30 ft., the accumulated remains of a still older civilization, that of the original neolithic settlers.

Already in the previous season test trenches had indicated the presence of this unparalleled deposit, and to make doubly sure I opened this season a wider terrace, contiguous with the main area of excavation, and invited Mr Miles Burkitt to come out and study this new culture on the spot. Every one interested will read his studied report with profit and enlightenment. Meanwhile he has favoured me with a précis of his observations.

He finds that the deposits below the painted pottery levels, roughly from the 30 ft. down to the 12 ft. level, represent two main phases of neolithic culture, in which black and brown burnished pottery vessels,

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sometimes decorated in simple ways, are associated with tools of obsidian, including a new type of beautifully made lance-head. A few polished celts also occurred. Our deeper soundings of the previous year had failed to find the beginnings of this culture at the present water level. 'Accepting the usual dating for the Tell Halaf culture', says Mr Burkitt, 'the base of the upper neolithic can hardly be later than 5000 B.C., while the lowest level reached must date to a thousand years earlier'.

In view of these promising results, and encouraged by the founder of the expedition which I have the honour to lead, I am now laying plans for a further three years' excavation, in the course of which we may hope to explore systematically the lower chalcolithic and the neolithic levels. I also propose to develop more widely the imperial Hittite levels in the hope, *inter alia*, of recovering some historical document which will tell us at least the original name of this most fertile site. In conclusion I would like to take this as the earliest opportunity of thanking the Turkish authorities for the great privilege of permitting the expedition to work in this area, and for the unfailing courtesy with which they have facilitated our task.

HOMO SAPIENS AT CHOUKOUTIEN (PLATES III-IV)

We are indebted to the Editor of THE TIMES for allowing us to publish the following communication from their Peking correspondent, 21 February 1939 (p. 13, col. 1), and for permission to reproduce the illustrations.

The hillside at Choukoutien, in which the famous 'Peking Man' was found, has yielded another anthropological treasure. The Peking Man was one of the most important discoveries of what is known as 'ancient man'; now, a few yards away, there have been found unusually interesting remains of 'modern man'—though 'modern' here means anything from 20,000 to 100,000 years old. They are the first remains of an Upper Palaeolithic population ever found in Asian soil, except in Palestine. The hillside at Choukoutien, looking out from the fringe of the North China mountains across the gigantic North China plain, is honeycombed with what the geologist recognizes as caves which time and the elements have filled with deposits of sand, earth, and small stones until they have become part and parcel of the hill. For some years investigators of several nationalities have been digging out these caves for the remains of man and the animals he hunted. Large blocks

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of material—earth, sand, and rubble in a hard compact—have been brought back to Peking to be broken down bit by bit in the laboratories of the Rockefeller Institute for the valuable fossilized remains they contain, and it is from some of these that have been taken the remains of late Palaeolithic man. (PLATE III).

They come from what is known as the Upper Cave, and consist of a 'population' of seven people, who appear to be members of one family: an old man, judged to be over 60, a younger man, two relatively young women, an adolescent, a child of five, and a new-born baby. With them were found implements, ornaments, and thousands of remains of animals, including bones or teeth of bears, hyenas, tigers, hunting leopards, and ostriches. The bears, hyenas, and ostriches belong to species which are now extinct, while the tiger and hunting leopard have long ago disappeared from this part of China.

Study of the remains has produced some remarkably interesting facts. To begin with, all seven people must have met violent deaths, for their skulls were clearly damaged by both blunt and pointed weapons while the scalp still covered the bone. Some of the skulls are badly smashed, but those of the old man and the two women are well enough preserved to permit determination of their special characteristics. They have certain facial features in common, but in some characteristics they differ so much that they give the distinct impression of belonging to three different racial groups which are now widely separated on the earth's surface.

The old man has for scientists a special interest. The brain case shows him to be of a very primitive type not very far removed from Neanderthal Man. In other features he recalls European man of the Upper Palaeolithic period, while his face rather suggests recent Mongolian types, though without being identical with any of them. His height has been judged as 5 ft. 8½ in.

Of the two women's skulls one looks very like that of a modern Melanesian woman of New Guinea, while the other is similar to that of a modern Eskimo woman. The skull of the 'Melanesian' woman has a depression round the top where the hair meets the forehead, similar to deformations found in Amerindians and also in Eastern peoples of later date. The Ainu woman has it, as a result of carrying her child on her back, suspended from a string or strap which passes round her forehead. (PLATE IV).

From these observations some interesting conclusions are drawn. In the first place Mongolian types such as are found in the modern

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North China population show no features pointing to ancestry among the population of the Upper Cave. Undoubtedly, says Dr Franz Weidenreich, who is the authority on the Peking Man, Chinese existed in this area at that time, and these seven people may have met their deaths at their hands. He believes that long before any immigration from Asia to the New World took place the types which now compose the American native population were settling down in or migrating through the eastern part of Asia. It may be, he adds, that anthropologists have been lucky enough at Choukoutien to catch some of the first Indians on their way by the land bridge to the New World.

These people had a relatively advanced culture, for found with them were stone implements, a bone needle, a bone implement, and ornaments made from beads of perforated teeth, sea shells, worked stones, and the bones of both birds and fishes. The bone needle, a finely shaped instrument but unfortunately broken off at the eye, argues that they wore sewn clothes. The beads included drilled stones coloured with hematite, the nearest known deposits of which are across 90 miles of mountainous country. The shells show that the cave-dwellers had also found the sea, now 125 miles away, and other things indicate that they had been south of the Yellow River, which is now 190 miles away. They were thus travelled people, as indeed their descendants must have been to find their way later down into North America before the Arctic and Pacific waters had met and the land bridge between Siberia and Alaska had been destroyed.

IRON AGE CAMPS

Dr R. E. Mortimer Wheeler writes :—To the March number of *ANTIQUITY*, pp. 58–79, I contributed a hastily written and still more hastily printed summary of the work of a recent expedition to Brittany and western Normandy. In the course of my remarks I referred to ‘the acid soils of the chalk and the green-sand’ as destructive of metal-work. The phrase was a startling one and should have been differently worded, since acid soils, though in place on the green-sand, are hardly ever found on the chalk. The fossil soils of Maiden Castle are in fact, neutral or alkaline, and the marked deterioration of metal-work on the site cannot therefore be so simply explained. Dr F. E. Zeuner, who has kindly reported on the matter, emphasizes the difficulty and complexity of the problem. It is conceivable, he points out, that, while the site was occupied, the upper layers containing the metal-work were full of decomposing organic detritus and had at that

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time a chemical reaction very different from that of a normal chalk soil, and may even have been acid. Alternatively, in decomposing habitation-strata ammonia is produced, and this might help to dissolve the bronze. A further alternative is that, some of the cultural layers being very loose, plenty of oxygenated water would come into contact with the metal, which thus would be oxydized more rapidly than in a denser medium. This would apply to the specimens in the loosely packed layers which are characteristic of a site such as Maiden Castle.

Whatever the cause, the solid fact remains that save in a few layers, generally of the more compact sort, the metal-work at Maiden Castle was in a singularly ill-preserved condition, and it is evident that not a little material of this kind must have perished completely.

COELACANTH FISH

The *Illustrated London News* of 11 March last published a magnificent double-plate of a very remarkable fish caught in a trawl-net off East London, Cape Province. The discovery of a living fish of the Coelacanth group, which hitherto was believed to have been extinct for fifty million years, is one of the most remarkable occurrences of natural history. The fish was described by Dr E. I. White, Deputy Keeper of the Department of Geology in the British Museum (Natural History). It was five feet in length, weighed 127 lbs., and a beautiful steel-blue in colour, with big dark blue eyes. The fish has been the subject of a paper read before the Linnean Society by Mr J. R. Norman, of the Department of Zoology in the British Museum, a summary of which was printed in *The Times* of 17 March (p. 25).

Reviews

KHORSABAD, Part I. By GORDON LOUD, with chapters by HENRI FRANKFORT and THORKILD JACOBSEN. (*University of Chicago, Oriental Institute Publications*, vol. XXXVIII). Cambridge University Press. 45s.

At the end of 1929 the Oriental Institute of Chicago began to excavate at Khorsabad, a place so famous that it would be an impertinence to offer it an introduction. Not merely was it in antiquity the creation and residence of the great Sargon II of Assyria, but it has acquired modern fame as the classic site where Paul Émile Botta in 1843 began the first excavations in the land of the Two Rivers, and therewith revealed the first glimpse of a civilization now seen to rank beside, if not before, the Egyptian in its significance for the growth of early man to human stature. Yet the mounds of Khorsabad had remained untouched since the departure of Victor Place and Félix Thomas in the 1850's, and the present volume is appropriately dedicated to these authors of the second of the two monumental works upon Khorsabad published in the nineteenth century.

The labours both of Botta and of Place have left much of the site unexplored ; moreover, the means at their disposal then were necessarily so inadequate that some of what they found might be expected to yield more information in the light of what has been learned since. Mr Loud and his collaborators have begun, in this volume, to publish matter in both of these kinds. A new gate was excavated in the *enceinte* of the city, and several rooms and appendages in the palace itself were re-excavated, with the expected result of a more complete revelation of their nature and contents, and incidentally with the very valuable accompaniment of fine specimens of sculptured wall-reliefs and figures, and interesting building-inscriptions of the royal founder. Among the most remarkable discoveries may be mentioned the king's throne-room with its sculptured stone dais for the throne, and the group of temples connected with the palace, remarkable for the curious decoration of their entrances. All of these features are fully described, with copious plans, drawings, photographs, and reproductions from the great folios of the pioneer explorers. Indeed, illustration is the essence of work at such a site as Khorsabad, and the authors deserve praise for their unstinted use of it, though it is true that some of the pictures of the work in progress are rather trivial, and it might be objected, in a similar vein, that something too much is made of the material difficulties of removing a winged bull to be taken to Chicago. Many such monsters were transported in the 'forties and 'fifties with immensely less mechanical aids, in far less settled conditions, and their hazards were then only beginning when they had been set

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a float upon the river ! Dr Jacobsen has published the inscriptions very competently, and all scholars interested in Assyrian art and history will look forward to further volumes upon the other notable discoveries known to have been made at Khorsabad. C. J. GADD.

ILE DE PAQUES. By HENRI LAVACHERY. *Paris, Bernard Grasset, 1935. pp. 299, and illustrations. 18 francs.*

In 1934 a Franco-Belgian expedition was sent to Easter Island. Dr Alfred Métraux, a Swiss by birth, represented France, and was concerned with ethnology and folklore. Dr Henri Lavachery represented Belgium, and worked at archaeology, and Dr I. Drapkin, a naturalist, was sent by Chili, to whom the island belongs. The expedition was on the island from July until December 1934. The prime mover in its inception was Professor Paul Rivet, of the ethnological department of the Trocadéro Museum, Paris (now rebuilt and called the Musée de l'Homme).

The book is almost a diary of the work of the author, 'Enlique' to the natives. He gives a list of former explorers of the island, and at first was much afraid that he would find nothing new to record. However, his thorough-going search with the help of the natives was well rewarded, and his conclusions do, to a large extent, uphold the views formed by Mrs Routledge and others.

These views are, shortly, that the inhabitants were originally Polynesians, and that they first occupied the island between the twelfth and thirteenth centuries A.D. Their dialect has affinities with that of the Maoris, and the invasion may have been roughly contemporary with that which peopled New Zealand and Hawaii. These islanders probably originated in the Gambier Islands, the nearest land on the west, whose monuments have closer affinities to the Easter Island 'ahus' than those of any other Polynesian peoples. The culture was not mixed ; there was an obvious unity manifested in the different arts, stone and wood, sculpture, rock carving, and tattooing. The size and peculiar nature of the great statues was no doubt due to the supply of easily worked stone from the quarries of the volcano Rano Raraku. Stone was considered an easier medium to work than wood, which is rare on the island. The habit of making statues to represent the dead is usual in Polynesia.

The chief monuments are the ahus, which are derelict stone erections, usually bordering the sea, the remains of sanctuaries, graves, and sometimes later villages built of the sacred materials, even of statues ; the statues themselves, both fallen from ahus and standing near the volcano ; petroglyphs, of which a number were sketched for the first time by M. Lavachery, and a few subterranean houses.

It is interesting to note that the method of inhumation was probably double,

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exposure of the corpse on the ahu, and subsequent burial of the bones. The cranium was sometimes preserved separately.

The volume is illustrated by photographs that we could wish were larger, and by excellent drawings of the petroglyphs. The appendices include useful statistics of population, a list of museums in which Easter Island antiquities are preserved, a list of the ahus, and a bibliography. D. P. DOBSON.

FURTHER EXCAVATIONS AT MOHENJO-DARO: BEING AN OFFICIAL ACCOUNT OF ARCHAEOLOGICAL EXCAVATIONS AT MOHENJO-DARO CARRIED OUT BY THE GOVERNMENT OF INDIA BETWEEN THE YEARS 1927 AND 1931. By E. J. H. MACKAY. *Delhi*, 1938. 2 vols. pp. xvi, 718, and 146 plates. £2 14s 3d.

The operations at Mohenjo-daro down to 1927 were fully described in the three volumes of *Mohenjo-daro and the Indus Civilization*, edited by Sir John Marshall. They sufficed to establish the general character of this completely forgotten civilization of the third millennium B.C. It might be thought that continuance of the excavations for another four years was a waste of money, and that their termination as a result of the financial crisis of 1931 was no great misfortune. Examination of Mackay's account of the last four years' diggings will refute such short-sighted suggestions. He was gradually recreating a complete city in a quite unprecedented state of preservation for such a remote period. As no decipherable literature has survived, we are forced to rely upon purely archaeological data for a reconstruction of the sociology and economy of India 4500 years ago. Only the complete exploration of a whole urban organism can supply the necessary evidence. The results here described already afford substantial help in reclothing with civic life the impressive ruins.

Mackay found a large complex of buildings which he interprets as a palace. If this designation be accepted, it introduces an important modification in the accounts of Indus sociology hitherto published. Appreciation of ancient town-planning is greatly enhanced by the discovery of the broad thoroughfares here described. To help us recapture a glimpse of their former busy life, the excavator happily photographed one of these streets, thronged with modern workmen and their carts. Another vivid touch is provided by his recognition of bazaar platforms built out on to the side-walks in front of the houses, precisely as is done in India today. Additional evidence for the proximity of the river is now forthcoming, but we are still left in doubt as to whether the city was fortified. Inconclusive indications of a circuit wall were coming to light when the excavations were closed down before they could be fully examined.

But excavation was not only recovering the plan—and all that the plan teaches—of one very ancient city, it was recovering also its forgotten history.

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Mohenjo-daro is truly 'a mound of many cities'—or to be exact several mounds. The accumulation of debris revealed in places exceeds 30 feet, and the limits of excavation were set, not by virgin soil which was nowhere reached, but by the level of standing water in the dry season. This accumulation had already been divided into Early, Intermediate and Late periods, each subdivided into three phases, numbered *from the top downwards*, I, II and III. The relics recovered illustrate a complete continuity of tradition in all periods, but it is here shown that the city had been devastated more than once by floods so disastrous as to entail its temporary desertion. The most serious came between Late III and Late II and between Intermediate III and Intermediate II. Apart from the havoc wrought by inundation, bodies of citizens, wounded with sharp implements and left unburied, tell of raids by hostile men, perhaps mountaineers from Baluchistan.

Stratigraphy not only reveals such episodes in the city's history, but also gives clues to social changes. During Late II-I houses were subdivided to accommodate more families. Whether this multiplication of dwellings indicates an absolute increase of the urban population, or merely crowding together of citizens under slum conditions in areas reconstructed after the flood, could only be determined when a larger sample of the inhabited area has been excavated.

The numerous relics recovered during the four years here surveyed not only widen our knowledge of familiar classes but introduce fresh types. Here Mackay describes and illustrates the first representations of boats from the Indus valley, the first model chariot, the first linear scale, locally made cylinder seals (from Late levels), true glazed pottery and reserved slip ware (from the Early period), candle-sticks very like those from Egypt, Crete and Thrace. Further indications of contact with more familiar civilizations to the west are afforded by the green steatite vase already published in *ANTIQUITY*, 1932, VI, 356, and found in an Early context, a marble seal of Elamite (or Assyrian) style from an Intermediate level, and a bronze axe-adze found in a Late house. Though it was found 6 feet down the author was at first inclined to regard the axe-adze as intrusive and possibly of Kushan date (p. 457), but revises this view on p. 640 and points out the implement's identity with those from Hissar III near Damghan in Iran. Etched carnelian beads are also treated as imports from Mesopotamia.

Plenty of relics are found lying about in the ruins and the list here given is enormous. Those hitherto recovered already suffice to give a vivid picture of the art and industry, the dress and furniture, even the toys and games of the Indus people. The relics, like the ruins, are effectively described by Dr Mackay, and happily interpreted in the light of his wide and intimate knowledge both of oriental life today and of the antiquities of Egypt and Mesopotamia.

The Government of India are to be congratulated on recognizing by the

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issue of this report that financial support of an excavation involves also responsibility for the proper scientific publication of the results. The British Museum would do well to follow this example. Eight years have elapsed between the termination of the excavations and the appearance of the report—as archaeological publication goes, this delay cannot be regarded as excessive. In any case it was not the author who was mainly to blame in the matter. But may we express the hope that an equally comprehensive account of the excavations at the sister city of Harappa will soon appear? The book has been printed in India. I have noticed no misprints and the type is easily legible. But the setting, notably of the title page, is not particularly pleasing. The 146 plates give excellent reproductions of fine photographs. Personally I should have preferred a larger scale for the architectural scenes even at the cost of some reduction in their number. In conclusion the book's appearance recalls once more the unprecedented opportunities Indian archaeology offers of rescuing from oblivion crucial pages in human history; for this, funds, which must be ample, seem the principal requisite missing. Dr Mackay at least is still very much alive, but his unique talents as an excavator in the Orient are lying idle.

V. GORDON CHILDE.

INCA TREASURE, AS DEPICTED BY SPANISH HISTORIANS. By S. K. LOTHROP. Publications of the F. W. Hodge Anniversary Benevolent Fund, vol. II. The South-Western Museum, Highland Park, Los Angeles, 1938. *pp.* VII, 75 *with 5 plates and 13 figures.* 2 dollars.

'The opulence of the Inca Empire [of Peru] and the richness of the loot secured by the Spaniards have long been fabulous'. Here we are given an excellent account of it, derived from first-hand sources, skilfully blended with the necessary amount of historical narrative. The treasure consisted largely of gold and silver objects, and its total value must have been enormous. Mr Lothrop estimates the value in modern currency of the loot of Cuzco, together with the ransom of Atahualpa, at nearly seventeen million dollars. 'So far as I am aware, not even one object from the loot of Peru has survived to this day. In 1535, Charles V issued a cédula ordering that all the gold and silver from Peru should be melted in the royal mints at Seville, Toledo, and Segovia. Subsequent shipments received similar treatment. Hence our knowledge of the wealth secured from the Incas comes entirely from historical narratives'.

To judge from the few illustrations, and from what has been since recovered by archaeological work, the artistic loss (as compared with the ethnographical) has not been very great; though one must not forget the portrait-headed vases of pottery. It is one consolation to know that much wealth was deliberately buried, and that by no means all of this was found by the Spaniards, in spite of all the tortures they inflicted upon the wretched owners. The record of the

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conquest of Peru is one of the darkest in all human history, and that is saying a lot.

The present admirably documented account gives the facts of the most celebrated and successful treasure-hunt of all time. O.G.S.C.

MAP OF PALESTINE OF THE OLD TESTAMENT. Published by the Survey of Palestine under the direction of F. J. Salmon, Commissioner for Lands and Surveys. *Jaffa, 1938. Price not stated.*

This is a small, single-sheet map, about 24 inches in length by 14 inches in breadth. It is on the scale of 1:500,000, or about 8 miles to an inch, and covers Palestine from Tyre to Beersheba, and from the Mediterranean to some ten miles east of Jordan.

The modern history of the identification of the place-names mentioned in the Old Testament begins with the researches of the American scholar, the Rev. Edward Robinson, who, in 1838, just over 100 years ago, commenced his investigations into the physical and historical geography of the Holy Land, which he published in 1841, under the title *Biblical Researches in Palestine*, with a further volume, *Later Biblical Researches in Palestine*, issued in 1856. Then, in sequence, we have the map of Lieut. Van de Velde, of the Dutch Navy, published about 1862. In 1865 came the foundation of the Palestine Exploration Fund and in 1882 that Fund issued a special edition of the *Map of Western Palestine* from surveys conducted by Lieuts. C. R. Conder and H. H. Kitchener; this special edition was printed to illustrate the Old Testament, the Apocrypha and Josephus. It is a large map on the scale of three-eighths of an inch to a mile, and is beautifully engraved by Stanford. It shows about 500 place-names.

In 1888 there was published by the same Fund a small book, entitled *Names and Places in the Old and New Testament and Apocrypha*. This contains 1150 names of places in Palestine, Mesopotamia, Sinai and Egypt, 'being it is believed all those that are mentioned in the Old Testament and the Apocrypha'. Of these names about 290 in Palestine and to the East of Jordan had not been identified, leaving some 500 sites identified in Palestine.

That classical work *The Historical Geography of the Holy Land*, by Sir George Adam Smith, was first published in 1894, and no fewer than twenty-five editions have made their appearance. It is indispensable for the study of the ancient geography of Palestine. It is illustrated by maps of the admirable type which we owe to the firm of John Bartholomew & Son. In 1915 there appeared the first edition of *The Historical Atlas of the Holy Land*, by the same author, with maps of the same character as the above, also by Bartholomew. The second edition is dated 1936. And lastly there is Père Abel's *Géographie de la Palestine*, in two volumes, of which the first was published in 1933 and the

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second in 1938; a work of great importance. The author remarks that *la masse des travaux d'approche allait s' éclipser devant les documents suivants de la cartographie moderne*, and cites the maps of Conder, Kitchener, Mantell, Schumacher, Newcombe, Musil, Guthe, Legendre and others, and the official surveys.

The useful little map under review is not meant to compete with the large, comprehensive works above mentioned. It is essentially a handy map for the traveller and tourist, giving on one small sheet the principal sites mentioned in the Old Testament. Actually about 280 place-names are given. A note is printed that some of the identifications are controversial; but the user may be assured that none is without authority, for the collaborators in the compilation are Père Abel, Dr Glueck, Professor Klein, the Department of Antiquities, and the British School of Archaeology. The great majority of the sites shown on this map are identical with those given in the 1882 map of the Palestine Exploration Fund. But there are some changes. For instance, Azekah, mentioned in one of the Lachish letters of 587 B.C., is marked a few miles to the south of Timnath; Lachish is definitely Tell-ed-Duweir; Gath has been moved 10 miles further to the north; and so on.

The map is surrounded by an attractive border, the decorative motives being derived 'from the ivories found in the ruins of Ahab's palace at Samaria'.

No text seems to have been issued to accompany the map. It may be hoped that such a text may be published with future editions, for it would add greatly to the interest and value of the map, and would serve to show what progress is being made in the century-old task of identifying the ancient place-names of Palestine.

C. F. ARDEN-CLOSE.

THE MEDITERRANEAN WORLD IN ANCIENT TIMES. By EVA MATTHEWS SANFORD. *New York: The Ronald Press Company. pp. 618, 64 plates and 11 maps. Price 4 dollars 50 cents.*

This attractively written compendium of Mediterranean History is intended for use in American Universities and should command a much wider public. Beginning with Palaeolithic man and ending with the decline and survival of Rome in the 8th century A.D. the book covers a tremendous period of history which is treated in a broad yet comprehensive survey.

The author has had the advantage of personal contact with many of the leading American and European scholars and has brought a balanced and impartial judgment to bear in the discussion of innumerable problems. This book fills a definite gap in the field of ancient history and particularly for the chalcolithic period synthesizes a great body of research, much of which is still only accessible in the scattered records of excavators' reports.

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The careful selection and variety of illustrations is most welcome, for many of them are likely to be new even to well-informed scholars. Occasionally the reproductions are blurred and hardly do justice to the subject, but it is perhaps too much to expect a very high quality of picture in a mass production of the kind. On the other hand the book is very well printed with clear subdivisions and a well thought-out scheme of arrangement. At the end of the volume there are excellent reading lists.

This work can be strongly recommended for teaching and as a useful handbook of reference, and we must congratulate the author on her courage and enterprise in tackling so vast a subject.

M. E. L. MALLOWAN.

THE EVOLUTION OF MAN AND HIS CULTURE. By H. C. BIBBY.
(The New People's Library, vol. xvi). *Gollancz*, 1938. pp. 93, *illus.* 1s 6d.

This little book is an attempt to give the Children of the Left a 'short, simple, authoritative' account of the evolution of man and of society. So far as it goes it is not misleading, judged by the standard of such productions; but it seems to the present reviewer to have two serious defects. It devotes a rather disproportionate amount of space to the earlier phases; and the account of the evolution of the State is definitely unsatisfactory. Why, for this latter, go to such sources as Perry and Engels when Professor Gordon Childe's authoritative books are available? (Engel's 'Origin of the Family' is described as 'a pioneer book, now rather dated but still invaluable'; but it was entirely second-hand, and based upon the work of Morgan). The author does not even seem to be aware of Childe's books, which are nowhere referred to. Yet Childe has presented a view of the early development of civilization which is not only regarded as authoritative by specialists but is also Marxist in the truest sense. For it applies the *principles* of Marxism to the results of modern archaeological research. We now know how and where the first great revolution (from nomadic to settled agricultural life) took place; and what were its social consequences. That is an important scientific achievement, putting completely out of date the earlier speculations (however legitimate and well founded) of comparative anthropology. The latter could only tell us how things *might* have happened; we are coming now to know how they *did*. The whole process consists of the interaction between first, man, and then organized groups of men, *i.e.* society, and the environment in which they lived. There is common ground, and the Marxists add (rightly, as we think) the class struggle. But, although the writer of this book quotes Marx, he is no nearer, we fear, to a proper understanding of Marxism in history than are most other Marxists. He does not realize that social history is the history of *tools* and of their effect on

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society ; and that what Engels said about the *gens* is far less important than what the archaeologist has found recently in Iraq, Egypt and India.

Nevertheless, we do not wish it to be thought that this book is without value. The writer has made a sincere attempt to convey some of the results of modern research to his readers, and he avoids many common errors. We criticize it rather on the grounds that it might have been very much better if written with a fuller and deeper comprehension, and if other sources had been used, as well as those (some of them excellent) quoted on the last page. O.G.S.C.

THE VICTORIA HISTORY OF THE COUNTY OF CAMBRIDGESHIRE AND THE ISLE OF ELY. Edited by L. F. SALZMAN. *The Victoria History of the Counties of England. Published for the University of London Institute of Historical Research.* Vol. I. pp. XIII, 436, 21 plates. Oxford University Press, 1938. £3 3s.

This fine volume forms a notable addition to the series of Victoria County Histories. The book, following the plan of the History as a whole, contains the accounts of the geology, botany and zoology of the county, with the record of early man, the history and archaeology of the Anglo-Saxon period and concludes with the Cambridgeshire Domesday Record.

The chapters on natural history, which form rather more than half the book, are written by specialists drawn from the University. The method of treatment varies in the different sections—thus in the geological chapter, compiled by the late Professor Marr and revised by Dr Dighton Thomas, there is a straightforward description of the stratigraphy of the county, with particular emphasis on the Pleistocene deposits. More extensive faunal lists would have been useful and have secured uniformity with the other natural history sections; there is hardly a mention of a single representative of the rich fauna of either the Elsworth Rock or the Cambridge Greensand. More serious is the criticism that the geological map does not incorporate the results of recent work, the outcrops of Ampthill and Oxford Clays are not separated, while reference to the geological map prepared for the Cambridge Meeting of the British Association 1938 reveals other differences. A comparison with that same map shows that, in so heavily drift-covered an area, stippling is a better method than layered colours of representing superficial deposits. Dr Godwin's treatment of the botanical section is on an ecological basis. It is especially here, in the discussion of the Fenland peats, that new evidence and new methods of investigation have contributed to the production of an account that could hardly have been written even twelve years ago. In by far the greater part of the Fens, drainage and agriculture have removed the uppermost layers of peat, so that only comparatively few isolated stretches of peat fen remain, of which Wicken and

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Chippenham are the largest. A photograph of vegetation in the former forms the frontispiece to the volume. The zoological chapter is a compilative work, the account of the Insecta naturally forming the largest part. Here research has hardly reached the stage to permit an ecological treatment and the greater part of this section is devoted to lists of species with notes on their occurrence and distribution, which show that all too many interesting forms have now vanished or become very rare.

Among archaeologists Cambridgeshire is famous for the influence of the early distribution studies of Sir Cyril Fox. Dr Clark's article on early man makes full use of this material, as well as of the results of later research for much of which he has himself been responsible. The rather meagre showing of the Palaeolithic period is due partly to lack of suitable exposures. For Mesolithic and later periods there is much more evidence within the county, though here the limitations associated with the somewhat artificial county boundaries appear, and the archaeology has often to draw on facts obtained outside Cambridgeshire. The important excavations of the Fenland Research Committee at the sand hillocks in the Ely fens have enabled Mesolithic, Neolithic and Early Bronze Age remains to be brought into relation with geological horizons. The results of a coordination of different methods of research are very apparent here, where a combination of the evidence from geological, botanical and archaeological lines of inquiry has resulted in much information about the character, chronology and environment of prehistoric cultures. The distribution-map shows in the Bronze Age a concentration of finds along the Fen margin, with a fair scattering over the chalk belt and the Fen islands; by the Iron Age the progressive submergence of the Fenland restricted occupation to the Cam valleys and the chalk belt in the southeast of the county.

Cambridgeshire is a very rich field for Anglo-Saxon archaeology and Mr Lethbridge divides this occupation into three sections; the Pagan Period comprising the fifth, sixth and early seventh centuries, which is partly overlapped by the Early Christian Period and lastly the Viking Age which continued to the eleventh century. The evidence for the first is abundant and comes largely from the numerous cemeteries distributed along the valleys. The great linear earth-works of the county which impede communication along the easy route of open chalk downland belong to the end of the Pagan phase. Later remains are more scanty, coming for the Early Christian Period mainly from the Burwell and Shudy Camps cemeteries, while for the Viking Age the series of weapons from rivers are the most notable finds. By the close of this period the villages had spread to the clay areas in the west of the county and the drift-covered slopes of the chalk, and their pattern as represented by the Domesday records is essentially the same as the present day village distribution. K. ST. JOSEPH.

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THE OLD WATER MILLS OF NORFOLK. By CLAUDE J. W. MESSENT.
With 60 pen and ink illustrations by the author. *Norwich : Fletcher,
Castle Works.* 1939. pp. 64. 6s 6d.

The water-mills remaining in Norfolk number 60, some of which have ceased working. Mr Messent has been at much pains to publish a drawing, with a short description, of each of the mills, and in his introduction gives some particulars of the building materials used, the most common form being timber framing covered with lapped weather boarding fixed horizontally. For others the red local bricks were used, and there is one mill, at Hingham, built with the local clay lump. A useful record of a fast-vanishing form of water power.

WEST COUNTRY. By C. HENRY WARREN. *Batsford*, 1938. 8s 6d.

Those who love to wallow in purple passages, those who deplore the removal of ivy from old buildings, leaving them 'nakedly clean like a spruced-up corpse' (p. 114), those who are strongly attracted by 'picturesque bits' and picture postcard country, thatched cottages and roses round the door, should not fail to read Mr Warren's book.

The book starts promisingly with an explanation of how a geological distribution-map helps the study of a district: this promise is not so apparent when it is found that there is no such map, or, for that matter, any map, in the book. A little further on, the reader must surely be astonished by the statement that Bath is essentially 'foreign'. It is not easy to gather on what the author bases his conceptions of 'English'; the inverted arches of Wells Cathedral are 'too ruggedly English for certain minds', and the George Inn, Norton St. Philip, is 'the very spit of what we mean when we say "an English house"'. Perhaps the ordered town, deliberately planned, is too elegant to belong to this land; or perhaps all that is truly English must belong to the 17th century and earlier.

In spite of the sententiousness worthy of a provincial evening paper, of the atmosphere as completely rustic as the much-deplored advertisements for 'This England' issued by a certain brewing firm, Mr Warren's phraseology and choice of epithets is not always quite happy. Personally we should have found some other way of saying: 'the tons of white dung with which the screaming gulls have *sugared* some of the rocks' (p. 89). The 'green earth' (p. 11) seems, too, somehow to savour of corruption and decomposition: or does our imagination carry us away?

The most attractive features of this book are those little touches of folklore, for which the author considers an apology necessary, and descriptions of local customs. The text is lavishly interspersed with photographs.

QUENTIN DOBSON.

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Editorial Notes

LAST June we spoke of the need for appointing immediately a Director of Antiquities for the Aden Protectorate, and we mentioned the fact that the Sudan Government had at last made a similar appointment for its own territory. For this good deed it deserves the thanks and congratulations of all archaeologists and anthropologists. But, good as it is, it does not go far enough. The Sudan covers a vast area. It is not, like Egypt, a narrow densely populated Nile-corridor, but a huge plain watered by annual summer rains and dotted with villages and granite 'gebels', standing out like pink islands in a sea of mimosa thorn. Throughout the whole of this area, more particularly in the valleys of the Nile and its tributaries, are the remains of ancient settlements. As cultivation spreads these are progressively endangered. Every day probably something irreplaceable is destroyed and a fragment of history gone for ever.



In the past the only archaeological post in the Sudan has been that of Conservator of Antiquities. The post was not a full-time one, the work being done in his spare time by a government official; the last three persons to hold it were the Director of Education, an Inspector of Education and the Government Geologist. With the creation of the new full-time post of Commissioner of Archaeology and Anthropology, that of Conservator of Antiquities has been allowed to lapse; but it is intended to revive it as soon as the country can afford it; and we may say that the creation of this post at the present difficult moment in its

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history reflects the greatest credit upon the Sudan Government. Those who know anything of the Sudan, whether directly or through the admirable medium of *Sudan Notes and Records*, realize that the Government is sympathetic and that it can be relied upon to carry out its intentions at the earliest available opportunity.

At present the duties of the Conservator of Antiquities are being performed by the Commissioner, but he is sadly overworked. In addition to supervising and helping the anthropological investigations of government officials, missionaries and specialists in the provinces, the new Commissioner is finding his time more than fully occupied in looking after the various museums (particularly the archaeological and ethnological collections) and the innumerable ancient sites—ancient Egyptian, Ethiopian, Meroitic and Christian—dotted at intervals of a few miles along the Nile Valley between Khartoum and Wadi Halfa; and there are many others in the valley of the Blue Nile and elsewhere throughout the vast Sudan.

The protection of these widely scattered sites is as urgent as it is difficult. The sites are looked on by the natives as convenient quarries for stone and manure (*marōg*); and often, where elaborate temples have been constructed of sandstone, the stone has become so impregnated with salt that exposure to the atmosphere by excavation has in many cases caused the rapid disintegration of the fabric.

There is a crying need for a permanent museum at Khartoum to house in a worthy fashion (so that they will be protected both from the climate and the ignorant, and also made available for study) not only the country's share of movable finds from past excavations carried out by the Harvard-Boston expeditions, Oxford Excavations in Nubia, and the Egypt Exploration Society and others, but also masonry of historical or artistic value which cannot be safely preserved on its original site.

Hitherto the country has had to be content with one room in Gordon College in which is displayed a small, very valuable, and as yet

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unpublished, collection of the smaller Sudanese antiquities dating between 1800 B.C. and A.D. 300. There is no room to display the additions to the collection that have been made in the last seven years : and these and other materials for student collections have been kept in boxes in temporary stores until such time as the country could afford a museum. It has however now been realized that it is impossible to keep many of the antiquities safely in boxes for an indefinite period. The packing and the boxes themselves deteriorate from the action of white ants, dry rot, etc., with resultant damage to specimens when moved ; and the damp atmosphere of the rainy season plays havoc with salt-impregnated stone and faience. The construction of a permanent museum being at present beyond the finances of the Sudan, temporary accommodation is being provided, so that next year it should be possible to make the whole archaeological collection safe from further deterioration, and available for students.



More than this however is needed ; a fairy godmother is urgently required by the Sudan, to provide it with a permanent museum with sufficient room to display on modern lines its unrivalled ethnological and archaeological specimens, to preserve for posterity representative pieces of masonry from the Egyptian, Ethiopian, and Meroitic sites of the country, and to enable gaps to be filled in the present collections, particularly in the representation of the prehistoric Stone Age cultures and of the Dark Age of the Sudan (*c.* A.D. 500-1500).

Such a gift would benefit not only the inhabitants of the Sudan, who are beginning to take an interest in their country's past, but also students of early African culture generally. In the past, waves of culture have repeatedly flowed through the Sudan into the lands that lie to the south and west of it : as was pointed out in a recent number of *ANTIQUITY*, in parts of the Sudan today there still survive (but only for a short time longer, in the face of the destructive advance of western influences) ancient customs and techniques that have already been obliterated in Egypt and elsewhere.



A permanent museum at Khartoum, if erected now, would ensure the preservation for posterity of such evidence of the past as survives to the present day. In a very few years it will be too late. The provision of an adequate staff and endowment would enable it to become a

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centre for the compilation, publication, and exchange of information about north-east Africa as a whole. The geographical position of Khartoum is ideal for this purpose.



We wish the Sudan Government every success in the forward move which it has made by appointing its first Commissioner for Archaeology and Anthropology. But progress must not stop there. More is required than one whole-time official for this vast area. An adequate museum and staff are the next essentials and they are long overdue. (A geological survey which will make its publications available to workers in neighbouring countries is also urgently required). Anyone desiring further information is invited to communicate with the Commissioner, Box 178 Khartoum, who will be glad to answer any enquiry. Here is a magnificent opportunity for a public-spirited millionaire or for one of the great endowed Corporations. We hope these words may meet the eyes of some such before it is too late.

Anglo-Saxon Ship-burial in East Suffolk

A most important archaeological discovery has been made in East Suffolk. A ship-burial of an early Anglo-Saxon leader has been found, dating from about A.D. 600. The funeral offerings were put in a great rowing-boat, which had been drawn up from the water and placed bodily in the trench-grave dug for its reception. The boat had a length of 82 feet and a beam of 16 feet. Nothing remains of it but a pattern of iron clench-nails in the ground; but grave-goods of great archaeological interest accompanied the burial. These are now in the care of the British Museum authorities. It should be added that the body itself has not been found.

By the kindness of the responsible authorities we hope to publish a full account of this outstanding discovery as an illustrated article in our December number.

Domesday Water Mills

by MARGARET T. HODGEN

THE staple food crops of Saxon and Norman England included rye, barley, oats, millet, beans, and peas. Wheat also was not unknown. In fact, a few carbonized grains have been found upon Neolithic sites.¹ But, even as late as the thirteenth century, wheaten bread in more than one region of England was a luxury food, enjoyed by the mass of the agricultural population only on boon days or religious festivals.² The porridges and cereal soups, or flat-baked cakes and small loaves, which accompanied the peasants' daily cheese, sausages or herring, were usually made from a coarse meal composed of rye, or rye mingled in some proportion with wheat, barley, oats or beans.

From time immemorial until well into the period of the Saxon settlement corn was ground with a saddle-stone or quern, which formed part of the hearthside gear of every farm-stead and was operated by the women of the family as one of their household tasks. At some time during the Saxon period, however, probably in the eighth century,³ a corn mill with mill stones turned by water power was introduced. Authorities are unable to agree upon the place of its first appearance in England⁴ or the place of its invention.⁵ Since none survives from this early date, they are also uncertain concerning many details of its structural design. But judging by water mills of a simple type which have been described by archaeologists as existing within recent years

¹ John Percival, *Wheat in Great Britain* (1934), 9-16; M. E. Seebohm, *Evolution of the English Farm* (1912), *passim*.

² Sir William Ashley, *The bread of our forefathers: an inquiry in economic history* (1928), 126 and *passim*.

³ Richard Bennett and John Elton, *History of Cornmilling* (1899), II, 96-7.

⁴ The earliest reliable allusion to the existence of a corn mill in England occurs for the year 762 in a charter granted by Ethelbert of Kent to the owners of a monastic mill situated east of Dover (Bennett and Elton, *op. cit.*, II, 96).

⁵ L. Lindet, 'Les origines du moulin à grains,' *Revue archéologique* (1899), t. II et (1900), t. I; Marc Bloch, 'Avenement et conquêtes du moulin à eau,' *Annales d'histoire économique et sociale*, septième année (1935), 538-63.

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in remote parts of Ireland, the Hebrides, the Shetland Islands and the Scandinavian peninsula,⁶ the Saxon mill, like these, was what is known as the Norse or Greek mill. The water-wheel lay horizontally in or upon the water, turning on a shaft fixed to a stone in the bed of the river. The upper end of the shaft passed through the lower of the two quern-like grinding stones, and the mill-wheel, the shaft and the upper stone turned together. Apparently these ancient mills were extremely small and adapted for use only on minor streams or rivulets.⁷ Mill stones were often no more than 18 to 36 inches in diameter and ground very slowly, one revolution of the water wheel producing but one revolution of the upper stone. Nevertheless, in 1086 there were over 5000 such mills in more than 3000 communities. A significant change had taken place in the material culture of England which removed the preparation of grain from the hearthside, altered the function of the housewife, introduced the miller and made the corn mill not only an important element in rural economy but a source of profit to those who erected them upon their holdings.

Where were these little mills located? Upon what streams did Britons begin their long history of the use of water power? In what regions or among what rural groups in Anglo-Saxon England did a far-reaching cultural and technological change of this character find welcome? Conversely, what areas were peopled by a folk less hospitable to the new device? The answers to such questions as these cannot fail to be of interest to archaeologists, historians, geographers, and economists. For the archaeologist and historian, they may throw some light upon debated problems of Saxon settlement and cultural change. For the economist and geographer, the distribution of Saxon water mills at the time of the Norman conquest forms a background against which to consider the utilization of water power in the later industrialization of England.

Domesday Book, referred to by Freeman as the first statistical document of modern Europe, is the obvious source to which those

⁶ Bennett and Elton, *op. cit.*, II, 6-30; Joseph P. O'Reilly, 'Some further notes on ancient horizontal watermills, native and foreign', *Proceedings of the Royal Irish Academy*, XXIX, Section C (1902-04), 55-84; E. Cecil Curwen, 'The Hebrides: a cultural back-water', *Antiquity*, vol. XII, no. 47 (1938), 284.

⁷ The mills in the Shetland Islands are described as 'being designed for a race of pygmies', while those existing in Scotland in 1814, of which there were more than 500, 'were enclosed in a hovel about the size of a pigstye, . . . and incapable of grinding more than a sack at a time'. (O'Reilly, *op. cit.*, 62-4.)

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interested in recovering some conception of the state of rural England at the end of the Anglo-Saxon period inevitably turn. This great inquest of 1086 was made by the commissioners of William I and 'had for its object to ascertain by local inquiry the amount at which each estate in the country was assessed to the land-tax called the Danegeld and . . . to obtain trustworthy data for a possible revision'.⁸ Domesday Book is therefore in modern terminology a rate-book or valuation list. Its compilation was conducted with all the tax-collector's scrupulous attention to fiscal detail; and 'so narrowly', laments a contemporary chronicler, 'did the Conqueror cause the Survey to be made that not a single hide, yard-land, ox, cow, or pig escaped registration'.

Owing to the right of multure by which peasants were theoretically compelled to bring their corn to be ground at the lord's mill, the Saxon water mills were regarded by the Conqueror as an important source of revenue, and his commissioners were instructed to adopt a common formula for investigation which involved a statement of their location, ownership and value. In making a return for the manor of East Hanney in Berkshire, for example, the following typical entry is recorded: 'The same count holds Hanlei (East Hanney). Two freemen held it T.R.E. It was assessed at six hides; now (it is assessed) at two hides. There is land for 5 ploughs. On the demesne there is 1 plough; and (there are) 20 cotters with 1 plough. One serf is there, and 2 mills worth (de) 27 shillings and 6 pence, and 70 acres of meadow . . .'.⁹ In some cases where the mill was divided among two or more holders, the mill entry was often expressed in fractions, thus: Methelwolde (Methwold, Norfolk) 'was held by Stigand T.R.E. (as) 20 ploughlands . . . Then as now $2\frac{1}{2}$ mills, 7 fisheries on the demesne'.¹⁰ Great care was taken by the commissioners to distinguish between mill rents paid in cash and rents paid in eels or farm products.¹¹ And some mills were mentioned as winter mills, an indication that in summer the wheels were compelled by the lack of water to remain idle.

Despite the care with which entries were made, however, and the wealth of cultural data available, difficult critical problems arise in any attempt to interpret Domesday statistics and arrive at strictly accurate

⁸ James Tait, *Domesday survey of Cheshire* (1916), 2-3.

⁹ *Victoria History of Berkshire*, ed. by P. H. Ditchfield and William Page (1906), I, 345.

¹⁰ *Victoria History of the County of Norfolk*, ed. by William Page (1906), II, 63.

¹¹ Rent was occasionally paid in iron, a fact which suggests that water power may have been used for purposes other than corn grinding.

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results. In so far as the water mills are concerned, the chief impediment to accuracy lies in the incompleteness, in the present state of Domesday scholarship, of the identification of eleventh-century place-names with their modern equivalents.¹² Although Domesday expository literature is large, the document has only in recent decades received the attention of highly trained philological investigation.¹³ Of the 34 counties covered by the Survey, only 28 have been recently edited and translated in such a way as to reduce errors in place-name conversion to a minimum.¹⁴ In order, therefore, to complete an enumeration and geographical distribution of the mills existing in 1086 for this study, an appeal has been made to earlier and other materials. In the case of Dorsetshire, Eyton's pioneer study of the survey for that county, made in the seventies of the last century, was used.¹⁵ For Wiltshire, W. H. Jones' *Domesday for Wiltshire*,¹⁶ published in 1865, was employed, while for Cambridgeshire, Gloucestershire, Oxfordshire and Staffordshire, the lists of Domesday mills offered in Bennett and Elton's *History of Cornmilling* were utilized in conjunction with county place-name studies indicating present-day spellings.¹⁷ As the result of this indirect procedure, the number of unidentified mill locations is somewhat high for several of the latter counties (Table 1), ranging from 6 per cent in Wiltshire to 12 per cent in Staffordshire. For England as a whole, however, the proportion of mills which cannot be located is only 2 per cent, or almost negligible.

The reader should be warned also that although an effort has been made to be exact, the occasional existence of the joint ownership of mills, expressed by the Domesday commissioners in fractions, may have led to error and could easily lead to difference of opinion. In the

¹² Sir Henry Ellis, *General introduction to Domesday Book* (1833), I, 41, n. 1.

¹³ Due largely to the admirable work of the English Place-Name Society.

¹⁴ These translations appear in the *Victoria Histories of the Counties of England* for Bedford, Berks., Buckingham, Cornwall, Derby, Devon, Essex, Hampshire, Hertford, Hereford, Huntingdon, Kent, Leicester, Norfolk, Northampton, Nottingham, Rutland, Shropshire, Somerset, Suffolk, Surrey, Sussex, Warwick, Worcester, Yorkshire.

A recent translation for Cheshire is in *Domesday survey of Cheshire* by James Tait (1916); for Lincolnshire in *The Lincolnshire Domesday and Lindsey survey*, translated and edited by C. W. Foster and Thomas Longley. Lincoln Record Society, 1924, vol. xix.

¹⁵ R. W. M. Eyton, *Key to Domesday* (1878).

¹⁶ W. H. Jones, *Domesday for Wiltshire* (1865).

¹⁷ Bennett and Elton, *op. cit.*, 147-162; W. W. Skeat, *Place-names of Cambridgeshire* (1901); H. Alexander, *Place-names of Oxfordshire* (1912); W. St. C. Baddeley, *Place-names of Gloucestershire* (1913); W. H. Duignan, *Staffordshire place-names* (1902).

DOMESDAY WATER MILLS

present study, wherever the sum of fractional mills on a site is a unit and clearly indicates the presence of a single mill, it has been so considered and so computed. On the other hand, where the sum is fractionally more or less than one mill, the assumption has been made that although the commissioners may have omitted to mention all mill holders, a fractional mention nevertheless indicates the existence of a mill and has been so counted.¹⁸ In other words, if a fraction of a mill was entered as subject to taxation, it has been assumed that a whole mill existed.

GEOGRAPHICAL DISTRIBUTION

Many students of the earlier periods of English history emphasize the influence of water supply upon human settlement. Man in the Bronze Age, according to Sir Cyril Fox, 'tended to live at the spring-line below the Downs, driving his herds and flocks on to the plateaux and hill-tops for pasture; while in the Age that followed he moved his actual dwelling sites on to the uplands, using them both for arable and for pasture and driving his flocks down to the spring-line for watering'.¹⁹ Other authorities assert that the river systems form a key by means of which the whole distribution of Anglo-Saxon settlements can be solved.²⁰ The Saxon invaders avoided Roman roads and penetrated the heart of England along the valley corridors, settling in villages higher and higher up the branching stream-lines.

¹⁸ The existence of fractional or jointly owned mills is discussed by Maitland in *Domesday Book and beyond* (144) as a possible evidence of communal ownership in eleventh-century England. (For a somewhat opposing view see Marcel Thévenin, 'Etudes sur la propriété au moyen âge; la "propriété" et la "justice" des moulins et fours', *Revue historique* (1886, xxxi, 241-58). On the other hand, fractional holdings in mills, like fractional holdings in fields and peasants, may be ascribed to division by inheritance. (G. G. Coulton, *Medieval village*, 1923, 40, 45.) Such mills are usually mentioned as being divided between two holders or in halves. Less frequently three or four holders are indicated by the mention of a third of a mill or a fourth of a mill. In Lincoln, Norfolk, Sussex, and Surrey however a few mills are divided among five, six, seven or even eight holders. Norfolk, Lincoln, Wiltshire, Suffolk, Kent and Hampshire are the counties in which the greater number of fractional mills occurred and, according to Maitland, the counties in which joint mill ownership was most frequent. In Norfolk 87 manors are mentioned as sites of fractional mills; in Lincoln 38; Wiltshire 23; Suffolk 15; Kent 13; Hampshire 11; Somerset 9; Oxford 7; Berkshire, Essex and Cambridge 6; Northampton, Sussex, Surrey and Nottingham 5; and the remaining counties less than 5.

¹⁹ Sir Cyril Fox, *The personality of Britain: its influence on inhabitant and invader in prehistoric and early historic times* (1933), 69.

²⁰ E. Thurlow Leeds, *Archaeology of the Anglo-Saxon settlements* (1913), 18.

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If the distribution of the 5624 Domesday mills is to be considered in any sense a clue to the boundaries of Saxon occupation, it should be noted (FIG. 1) that while mills were common enough on the streams south of the Severn and Trent, they are mentioned very infrequently on the rivers of northern England and almost not at all in Cornwall.²¹ Apparently the Saxon utilization of water power began very abruptly in the west at a line formed by the river Exe and Exmoor Forest,²² and from there followed a pattern dictated by the eastern radiation of fan-like water-sheds from the central elevation formed by the Mendip Hills, Salisbury Plain, and the Whitehorse Hills (Berkshire and N. Wilts Downs). The greatest mill density lay around this central plateau, in Wiltshire, Gloucestershire and Hampshire, and at the eastern termini of the fan spokes facing the continent, particularly in Lincolnshire, Norfolk and Kent. It should be observed also (although small-scale maps fail to present the facts clearly) that the banks of the three great highway rivers, the Severn, Trent and Thames, were seldom regarded by mill builders as suitable sites for milling operations. The lower waters of the Severn, for example, were free of mills as far as Tewkesbury. On the Trent, the first three mill wheels appear at Grassthorpe thirty or forty miles from its junction with the Humber. On the Thames the first mill upstream appeared possibly at Staines but more probably at Basildon.

On the tributaries of these three main waterways, however, and on the numberless smaller, shorter streams fringing the coast line of England, east, south, and west, the story was different. Every northern affluent of the Thames, with the exception of an occasional river in Essex, bore at least a few mills upon its banks. The group of water-courses flowing southeast from the Cotswolds, including the Cherwell, Ray, Glyme, Dorn, Evenlode, Sars Brook, Windrush, Sherborne, Dikler, Leach, Coln and Char turned many wheels. (FIGS. 1 and 4.) South of the Thames, the short streams draining the North Downs in Kent carried the majority of mills in that rather densely milled county, while further west the Kennet, Lambourne, Enborne, and Loddon were thickly milled. The water power afforded by the complex of river

²¹ A number of mills are mentioned as existing on manors which now seem to be unwatered. It has been suggested that these may have been operated by horse power. It is more likely that changes in rainfall have occurred leading to the drying up of formerly water-filled valleys. G. M. Meyer, 'Early water mills in relation to changes in rainfall in East Kent', *Quarterly Journal of the Royal Meteorological Society* (1927), LIII, 407-19.

²² R. Pearse Chope, 'Domesday mills in Devon and Cornwall', *Devon and Cornwall Notes and Queries* (1922), XII, 21-23.

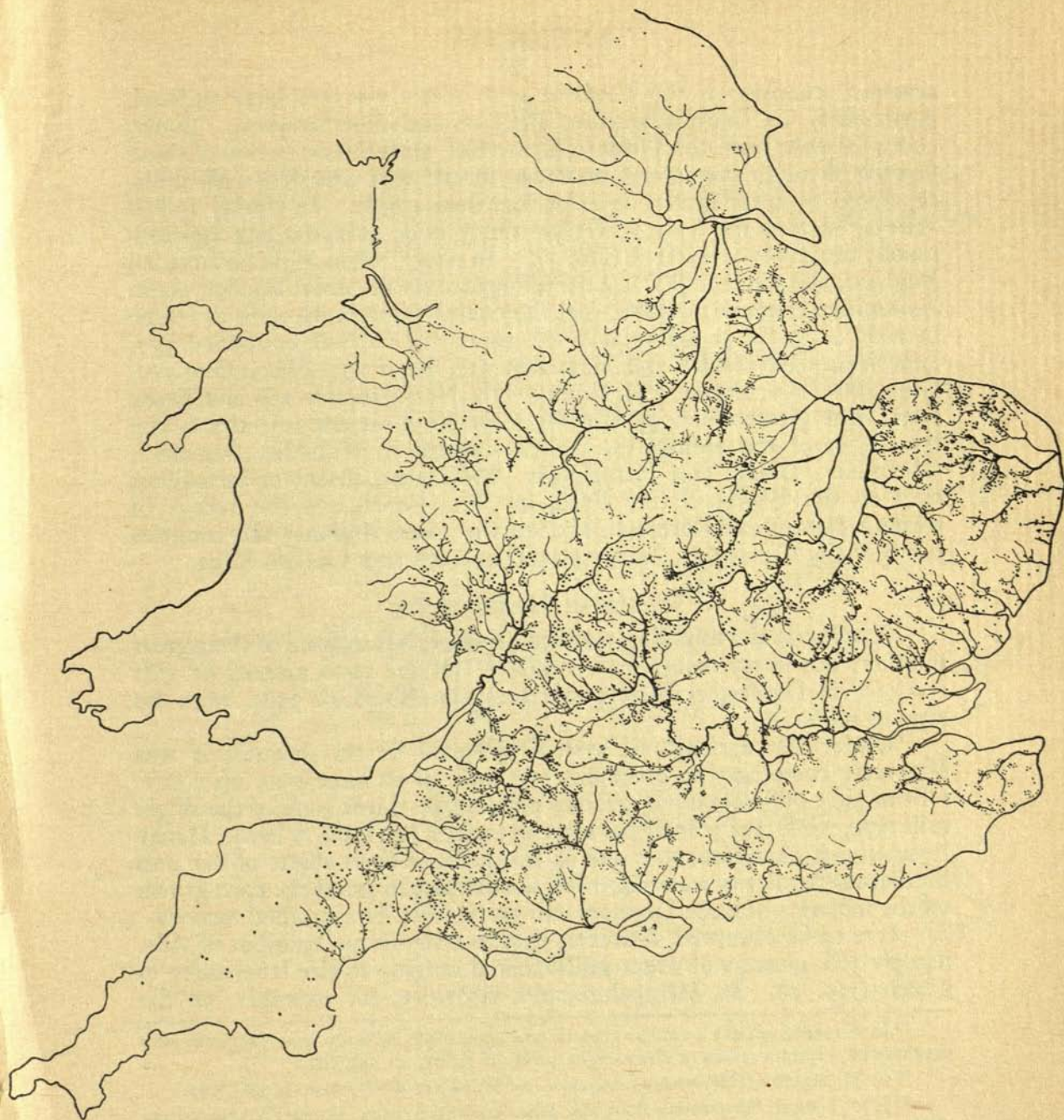


FIG. 1. SKETCH MAP OF DOMESDAY WATER MILLS AND RIVER SYSTEMS

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systems centring on the Fenland and Wash was similarly utilized, particularly in Cambridgeshire, Bedford and Northampton. Every coastal stream from the Wash to Lyme Bay, with the exception of those flowing through marshland or dense forest, was crowded with mills, separated in innumerable cases by less than a mile. In thickly milled valleys, such as the Vale of Wylye, thirty mills existed along approximately ten miles of water.²³ (FIG. 3). In short, when William invaded England, the water mill was a commonplace on the great English plain. Assuming, as seems possible, that the county boundaries were the same in 1086 as at the present day,²⁴ the county of Norfolk contained 537, Lincoln 436, Wiltshire 390, Somerset 351, Kent 334, Hampshire 320, Gloucester 250, Dorset 226, Suffolk 218, Northampton 206 and Essex 203. The greatest density of mills occurred in eastern counties facing the continent and around the central uplands in Wiltshire, Somerset, Gloucester, Dorset and Hampshire. Mills were absent or infrequent only on the Weald, in the Fens, in New Forest, on the moors of western Devon and Cornwall, and on the rivers draining the counties north of the Cotswolds, Northampton uplands and Lincoln Edge.

MANORIAL DISTRIBUTION

A mill was the unfailing mark of a manor, but in spite of their great number, not every manor had a mill.²⁵ Of the 9250 manors or villas recorded in Domesday Book²⁶ only about one-third, or 3463, were the sites of mills.

Under the agricultural system reflected in the Inquest it was obviously customary to erect but one mill, or at most two, on a site. Of the 3463 milled manors, 2220 or almost two-thirds were of the single mill type, while 2957, or 85 per cent, possessed two mills or less. It may be assumed, therefore, that one or two mills of even slight power and indifferent regularity were regarded as adequate to grind the corn grown on the majority of milled manors and on the near-by unmilled manors.

It is to be observed, however, that in a significant number of villas, namely 506, clusters of water mills existed ranging in size from three to fifteen (FIG. 2). In Hampshire and Wiltshire, for example, on the

²³ In Scotland, where a similar type of mill abounded, no fewer than fifty-one were enumerated within a radius of about eight miles (O'Reilly, *op. cit.*, 82).

²⁴ F. M. Stenton, *William the Conqueror and the rule of the Normans* (1908), 489.

²⁵ J. H. Round, 'Domesday Book and other kindred records', in the *Victoria History of Devonshire* (1906), I, 399.

²⁶ H. de B. Gibbins, *Industry in England* (1920), 70.

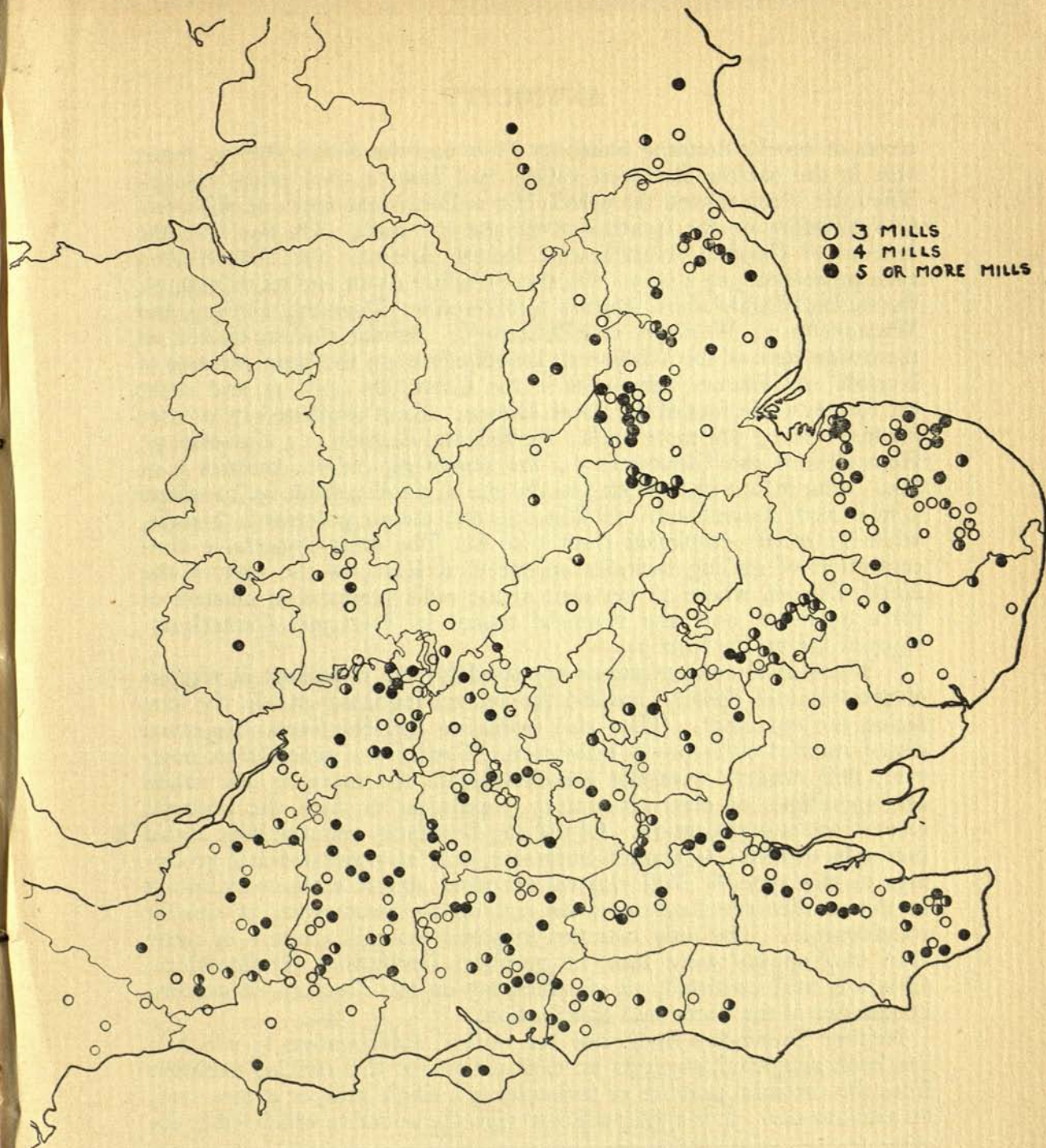


FIG. 2. COUNTY DISTRIBUTION OF WATER MILL CLUSTERS

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rivers or brooks draining Salisbury Plain and the North Downs, many villas in the narrow sheltered valleys had from 4 to 9 mills (FIG. 3). The little Meon turned the wheels of 7 at East Meon and 4 at Soberton. Old Alresford on the Itchen was the site of 9 mills. On the Test, the manors of Overton, Hurstbourne Priors, Andover and Abbots Ann each possessed 3 or 4 or 5. On the Wiltshire Avon and its tributaries, the mills of Donhead St. Andrew numbered 8; Downton, Pewsey, and Warminster 7; Westbury and Potterne 6. Similar clusters existed on the headwaters of the Witham in Lincolnshire, on the short streams of Norfolk, on Thames tributaries in the Cotswolds (FIG. 4), and along the Rother at the foot of the South Downs. Kent contained 17 or more manors bearing 5 or more mills; Norfolk and Lincoln 12; Somerset 9; Hampshire 8 and Gloucester 7; the remaining milled counties 5 or less. One manor in Norfolk was the site of 9 mills, while on 11 others 5 to 8 were maintained. In Lincoln, one manor possessed 12 mills, while 11 others employed from 5 to 8. The most impressive concentration of milling facilities appeared in Kent, at the foot of the North Downs, where 26 per cent of the mills occurred in clusters of three or more, and one manorial tenant in Westgate, Canterbury, enjoyed the profits from 15.

These local concentrations appeared for the most part in regions of greatest mill density, around the central highland and in the east facing the continent. They also underline and emphasize the radial distribution of water-power utilization. Contrary to expectation, however, mill clusters were not associated with the more or less urban agglomerations of eleventh-century population or with the borough form of social organization. Of the 103 Domesday communities, styled boroughs or stated to contain burgesses, only 16 possessed mill groupings of this type.²⁷ Mill clusters occurred almost without exception in the smaller rural communities and on the headwaters of smaller watercourses. The only counties in which no mill clusters of more than two appear were four in number, Derbyshire, Staffordshire, Cheshire, and Cornwall, all of which lay on the northern or western boundaries of the water mill distribution.

Neither Domesday Book, nor the critical investigation to which it has been subjected, suggests an explanation for this striking variation from the common practice of maintaining a single mill, or at most two, on a manor site. It is a temptation to view the initiative which led to the

²⁷ Adolphus Ballard, *Domesday boroughs* (1904), 9-10.

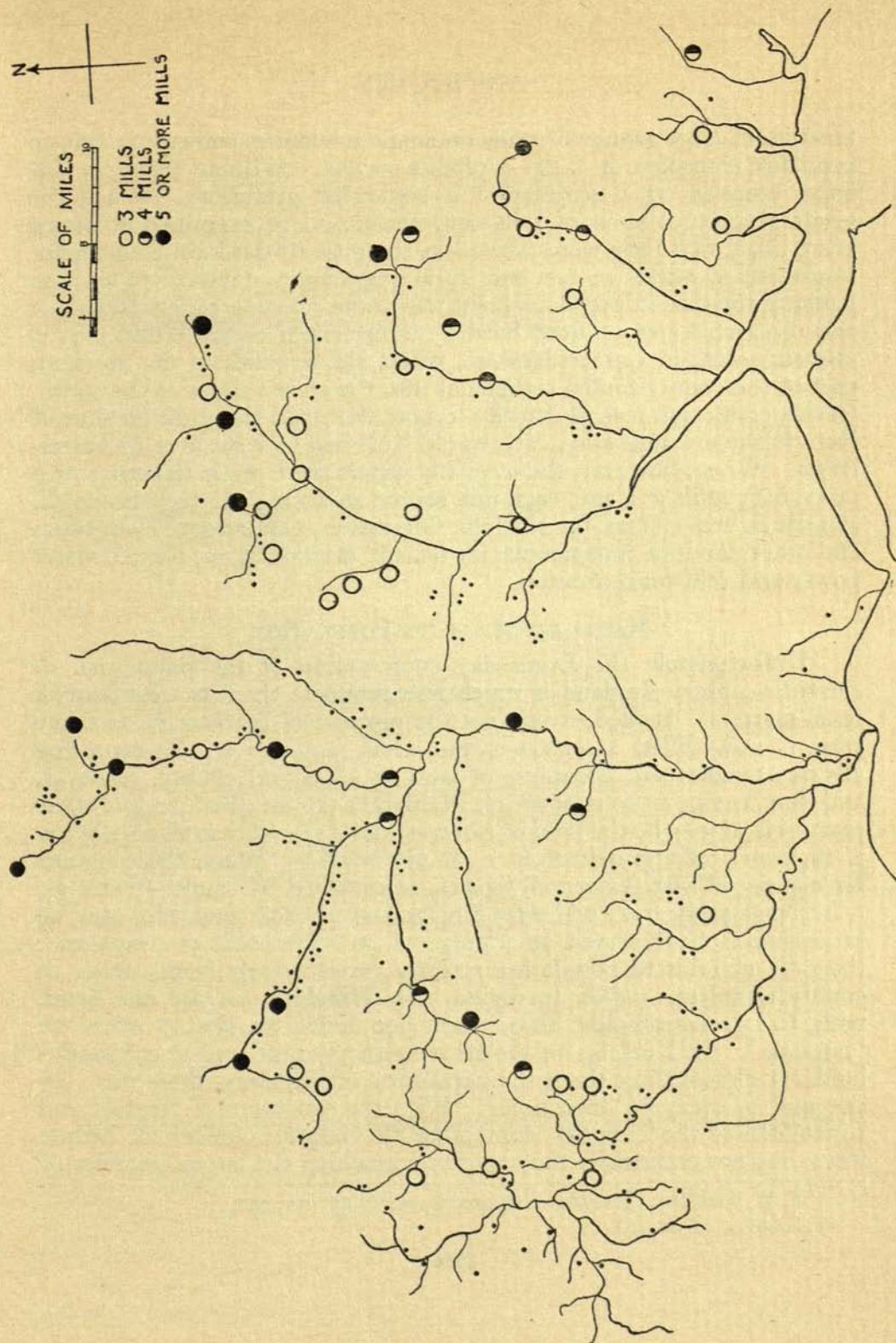


FIG. 3. WATER MILL CLUSTERS ON THE STOUR, HAMPSHIRE AVON, WYLYE, TEST AND ITCHEN RIVERS

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erection of large groups of mills on single manors as something akin to capitalist enterprise in a pre-capitalist society. Without further evidence, however, this explanation is somewhat gratuitous. When the geographical positions of mill concentrations are examined it seems more likely that they were erected in response to the local coincidence of restricted water power and rural population density. This was patently the case in populous Kent where the rivers were few and short and the arable area reduced by the encroachment of the Weald. It is also suggested in Cambridgeshire, where the marshes in the northern part of the county forced settlement into the river valleys of the south. In this connexion it is of interest to note that, had the 130 mill sites of Kent borne one mill only, each would have had to serve over 90 households. When, however, the available waters were made to turn 3 or 4 or even 15 mills at a site, each mill served an average of 36 households. Similar figures emerge for Norfolk, Gloucester, Cambridge, Hampshire and other counties conspicuous for density of population, limited water power and mill concentration.

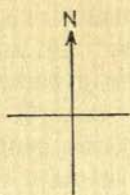
RATIO OF MILLS TO POPULATION

Unfortunately the Domesday enumeration of the population of eleventh-century England is much less complete than its treatment of fiscal matters. Indeed, it was not the purpose of the Inquest to make what is now called a census of the whole population. In reporting 287,045 tenants and occupiers of land by class and county, however, and thus leaving us to suppose, in Maitland's phrase, that 'each of these persons is or may be the head of a household' of five,²⁸ a total population is suggested of approximately 1,400,000 souls.²⁹ When these figures are compared with water mill figures, as classified by county (Table 1), it becomes plain that, while for England as a whole each mill may be presumed to have served an average of 50 households or 'families', the ratio of mills to population actually varied widely from county to county and from region to region. In Wiltshire, on the one hand, with 10,150 households, there were 390 mills, or one to every 26 'families'. In Norfolk, on the other, with 537 mills and 27,078 households, or almost three times the population of Wiltshire, there was only one mill to every 50 households. With the exception of Norfolk and Lincolnshire, the counties containing the largest number of people were thus not necessarily the counties containing the largest number of

²⁸ F. W. Maitland, *Domesday Book and beyond* (1897), 17, 437.

²⁹ Gibbins, *op. cit.*, 66.

SCALE OF MILES
1 5 10



- 3 MILLS
- ◐ 4 MILLS
- 5 OR MORE MILLS

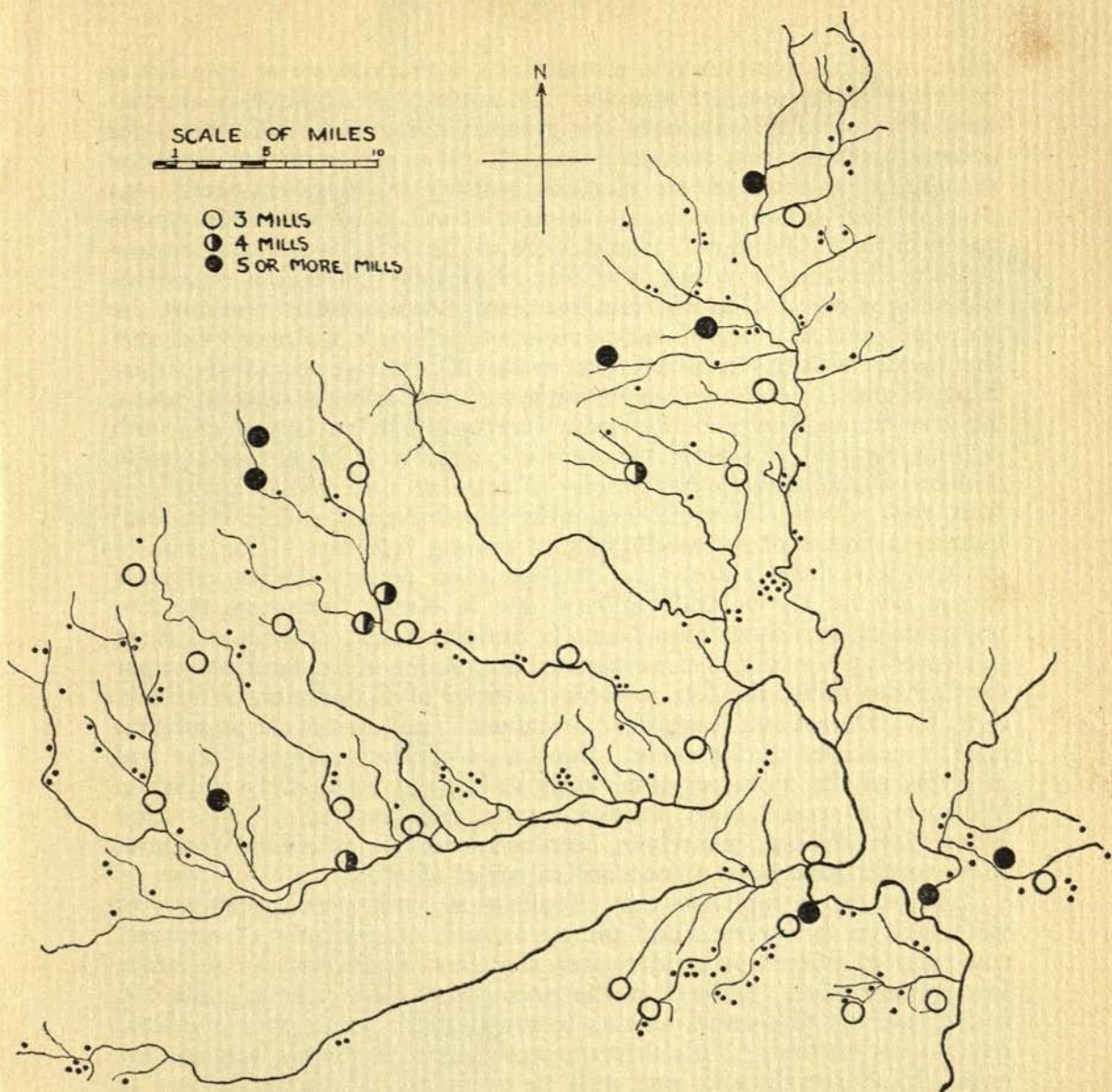


FIG. 4. WATER MILL CLUSTERS ON THE TRIBUTARIES OF THE THAMES IN THE COTSWOLDS

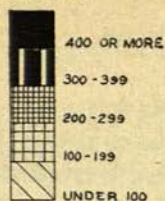
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mills. Suffolk, for example, stands third in the number of households but ninth in the number of mills. Or, stating the situation in another way, the counties containing the greatest number of mills were not necessarily those most adequately milled. It has been asserted that the average population of an eleventh-century village was about 150 persons³⁰ or 30 households, an estimate which, even in Wiltshire with one mill to 26 'families', would leave six households per mill unprovided with access to milling facilities. But if, for the sake of argument, the ratio of one mill to 26 'families', which obtained in Wiltshire, be accepted as a measure of the number of mills just necessary to meet the needs of a given population without recourse to querns, then Norfolk and Lincoln, with a somewhat greater number of mills and a far greater number of households, were only half as well served with milling facilities. In fact, if the maps A, C, and D in FIG. 5 be compared, it will be seen that, while the density of population decreases as we pass from east to west, the density of mills (Norfolk and Lincoln excepted) increases, and hence the adequacy of milling facilities. The areas of greatest mill density and greatest population density do not coincide except around the central highland and in Kent. Although the five populous eastern counties of Norfolk, Suffolk, Essex, Lincoln and Kent possessed 35 per cent of the estimated population of England and 30 per cent of the mills, the five western counties of Gloucester, Wiltshire, Somerset, Dorset and Hampshire contained 17 per cent of the population and 27 per cent of the mills. Such a comparison suggests that had Norfolk, as the most populous county, been as adequately milled as Wiltshire, it would have possessed 1100 mills instead of 537. Had Domesday England, as a whole, been as adequately milled as Wiltshire, there would have been 12,000 mills instead of 5624.

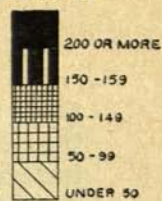
There can be no doubt that, in spite of the widespread acceptance of water-driven mill-wheels and the application of the right of multure, some use of querns or saddlestones remained a necessity in all but a few communities. Indeed, in the presence of a shortage of mills, the insistent effort to compel tenants to grind at the lord's mill is a little hard to understand. This shortage was marked in the far west and far east. In Cornwall with one mill to every 1087 households, and in Devonshire with one to every 200, the use of the quern must have been practically universal. To a lesser degree, the same statement can be made for the eastern counties of Suffolk and Essex, with 93 and 79

³⁰ Gibbins, *op. cit.*, 81 and note 3. See also Maitland, *op. cit.*, 17-20.

NUMBER OF MILLS
BY COUNTIES



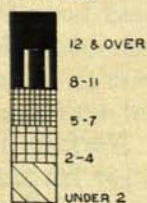
NUMBER OF MILL SITES
BY COUNTIES



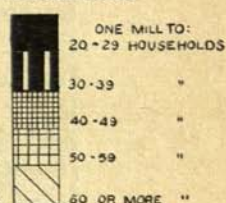
A

B

RECORDED ADULTS
PER SQ. MILE



RATIO OF MILLS
TO HOUSEHOLDS



C

D

FIG. 5

ANTIQUITY

respectively. The only rural groups which took definite advantage of the new device and were by that token most hospitable to cultural change in Saxon England, were those which lived around the central uplands and in Surrey and Kent.

Whether or not these regions in which querns were least necessary were also the areas in which the water mill was first introduced is a question of some interest, but one for which dated history has no answer. Many anthropologists, confronted by such a problem, would be tempted to interpret geographical density of trait-distribution as an indication of prolonged trait use, and thus suggest that the water mill first appeared in Lincolnshire and Norfolk from a possible continental source. Or observing a similar density in the Salisbury Plain region, they might surmise that the mill had been twice introduced, once from the northeast and again from the south, each introduction leading to a diffusion into surrounding areas.

But the age and area hypothesis, as applied to cultural facts, has never received adequate historical verification and conceals many pitfalls for the unwary. If then it be deemed necessary to locate the region in eleventh-century England in which the water mill had been longest known or earliest employed, it would seem wiser to appeal to an argument based upon the distributions of mill clusters and jointly owned mills. Assuming with respect to mill clusters that a second mill would be built on a site only after a first one had been found useful, and that therefore two-mill sites were older than one-mill sites, and assuming further that sites bearing 5 to 15 mills were far older than sites bearing a lesser number, the conclusion is reached that the water mill appeared first in Kent, with Lincolnshire and Norfolk following soon after. (Table 2). Or assuming jointly owned (or fractionally stated) mills to be the outcome of division by inheritance, and the areas bearing the greater number of such mills to be those in which a relatively long period of division had existed, Norfolk, Lincolnshire and Wiltshire again loom very large.

Indeed, all arguments agree in pointing to the Salisbury Plain region as one in which the water-driven mill had been employed long enough to lead to marked indications of capitalist enterprise and inherited ownership. While for similar reasons the eastern counties of Kent, Lincolnshire and Norfolk appear to have been equally responsive to cultural change in the period just antecedent to the Norman Conquest. Our knowledge of the process of change in human activities is too rudimentary to afford an explanation of why these areas were the

DOMESDAY WATER MILLS

first (if they were) to introduce the water mill, or why a useful device should have been welcomed early by some groups of rural folk and rejected by others. We have no way of knowing yet why an element of advanced culture was immediately incorporated into the life of one agricultural community and resisted by an adjoining one. It is suggested, however, that this difference in initiative among human beings is of crucial importance in accounting for technological advancement as well as social 'progress'.

TABLES showing the number of water mills, heads of households, households per mill, and sites in the counties of Domesday England are printed on pages 278-9.

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TABLE 1

Showing the number of water mills, heads of households, and households per mill in the counties of Domesday England

Counties	Number of Mills			Number of	
	Unidentified	Identified	Total	Heads of Households*	Households per Mill
Norfolk		537	537	27,087	50.4
Lincolnshire ..	24	412	436	25,305	58.0
Wiltshire	10	380	390	10,150	26.0
Somerset		351	351	13,764	39.2
Kent	20	314	334	12,205	36.5
Hampshire		320	320	10,373	32.4
Gloucester	27	223	250	8,366	33.4
Dorset	2	224	226	7,807	34.9
Suffolk	12	206	218	20,491	93.9
Northampton ..		206	206	8,441	40.9
Essex	3	200	203	16,060	79.1
Sussex	6	158	164	10,410	63.4
Oxford	6	156	162	6,775	41.7
Yorkshire		145	145	8,055	54.8
Berkshire		135	135	6,324	46.7
Nottingham		128	128	5,686	44.4
Cambridge	6	119	125	5,204	41.6
Buckingham	1	125	126	5,420	43.3
Leicester		123	123	6,772	54.6
Surrey		120	120	4,383	35.6
Warwick	3	114	117	6,574	57.6
Hereford	2	96	98	5,368	54.1
Worcester		97	97	4,625	46.7
Bedford		93	93	3,875	42.6
Shropshire		88	88	5,080	56.5
Devonshire		87	87	17,434	200.1
Hertford		82	82	4,927	60.0
Derby		68	68	3,041	43.2
Stafford	9	59	68	3,178	46.7
Middlesex		36	36	2,302	63.9
Rutland		31	31	862	27.8
Huntingdon		31	31	2,914	94.0
Cheshire	1	23	24	2,349	97.8
Cornwall		5	5	5,438	1087.6
Total	132	5,492	5,624	287,045	

* Sir Henry Ellis, as cited in H. de B. Gibbins, *Industry in England* (1920), 67.

DOMESDAY WATER MILLS

TABLE 2

Showing the number of identifiable water mill sites and the number of mills per site in the counties of Domesday England

Counties	Number of Mill Sites Bearing															Total Sites
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	
Norfolk ..	160	93	29	8	6	2	1	1	1							301
Somerset ..	162	51	7	3	4	2	2		1							232
Lincolnshire ..	114	56	15	16	3	4	3	1				1				213
Wiltshire ..	111	52	8	5	2	3	3	4	2	1		1				192
Hampshire ..	98	42	20	7	1		2	2	1							173
Dorset ..	119	31	9	2				1								162
Northampton ..	105	39	5	2												151
Suffolk ..	115	24	6	5	1											151
Essex ..	116	29	3	1	1			1								151
Kent ..	62	33	12	5	6	4		2	1	2	1				1	129
Gloucester ..	58	22	17	6	3		1	1		1						109
Yorkshire ..	86	11	6	2				1								106
Oxford ..	59	23	7	1	4	1										95
Sussex ..	60	13	6	4	2			1	1			1				88
Warwick ..	68	16	2	2												88
Buckingham ..	62	15	5			1		1								84
Leicester ..	59	20	2	1			2									84
Nottingham ..	51	24	2	1	1	1										80
Berkshire ..	49	19	9		1	1	1									80
Shropshire ..	69	8	1													78
Devonshire ..	62	8	3													73
Hereford ..	56	10	1		1			1								69
Worcester ..	48	8	8	1	1							1				67
Surrey ..	41	16	5		1	2	1	1								67
Derby ..	50	9														59
Bedford ..	35	19	2	2		1										59
Stafford ..	43	8														51
Hertford ..	32	12	3	3	1											51
Cambridge ..	20	10	9	8	1		1	1								50
Cheshire ..	19	2														21
Huntingdon ..	11	7	2													20
Rutland ..	11	3		2		1										17
Middlesex ..	6	3	2	3		1										15
Cornwall ..	3	1														4
Total Identifiable Sites ..	2,220	737	206	90	40	24	17	19	7	4	2	3			1	3,370
Unidentifiable Sites ..																93
Total Sites ..																3,463

Air Reconnaissance of Roman Scotland

by O. G. S. CRAWFORD

ON 6 June I started from Southampton to carry out an investigation of the Roman roads and sites in Scotland from the air. For several years, as part of my official duties, I have been examining these on foot, in pursuance of a plan to publish a third edition of the Ordnance Survey Map of Roman Britain.* It is intended to prepare the way for this map (which may be published on a slightly larger scale—10 miles instead of 16 miles to the inch) by the publication of maps of special regions on the scale of 4 miles to the inch. The drawing of the first two of these (Scotland, Sheet 3 [Forth and Tay], and Sheet 1 [The Border]) had been finished; but many doubtful points remained even after intensive field-work, and it seemed probable that a short air reconnaissance under favourable conditions would solve some of them. This opinion was amply justified by results. About a dozen new Roman sites (including as 'sites' new stretches of Roman road) were discovered; about 50 new sites in all, including many native forts, were placed on the map; and valuable results of a general character were obtained.**

June was chosen because then crop-sites, revealed by growing corn, are seen at their best. Last June was a particularly favourable month for the work, for a long drought had burnt up the grass leaving untouched the silted-up ditches, pits and post-holes which looked as if they had been painted in vivid green on a brown canvas. We were fortunate in catching the last week of this drought; the weather broke on the day that we flew south. We were thus able to observe and photograph the sites under the best possible conditions. Furthermore, work of this kind cannot be carried out continuously in a mountainous country like Scotland except during a period when clouds are high or absent. It is not often that one can fly round the Devil's Beeftub and Hartfell day after day under such perfect conditions.

The work was made possible by the enterprise and generosity of Mr Geoffrey Alington, of Air Touring; many of the new sites were in fact seen first by Mr Alington, who flew the aeroplane (a Puss-Moth)

* See note on page 292.

** My own observations have been made over, for publication in greater detail, to Mr K. St. Joseph, who is undertaking trial excavations for the Glasgow Archaeological Society at a number of the sites, as part of this Society's scheme of investigation of Roman works in southwest Scotland.

PLATE I

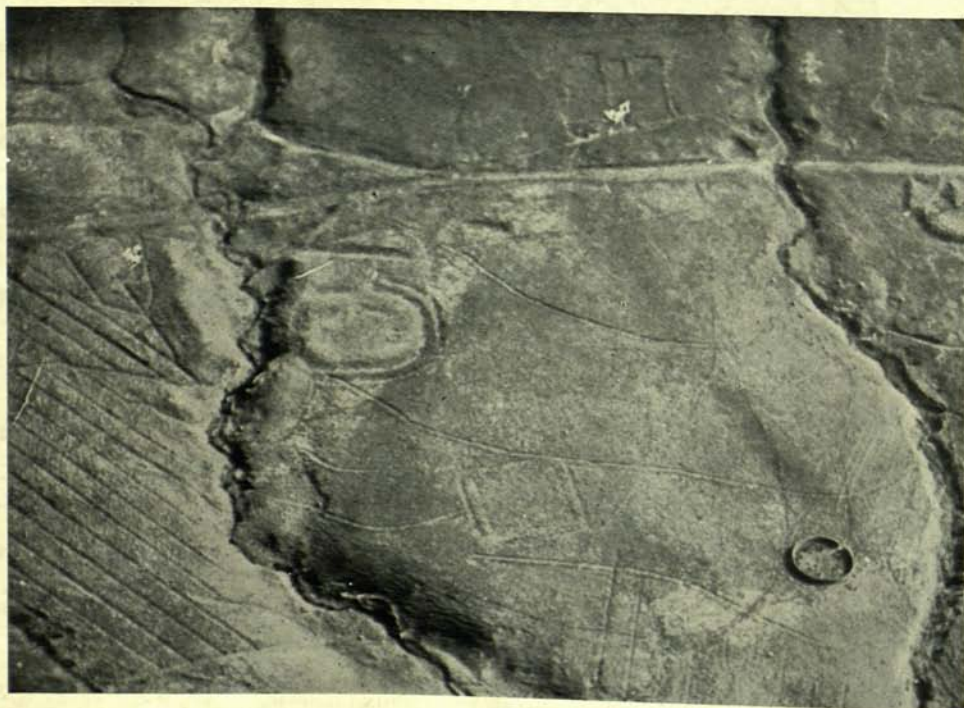


EDINBURGH FROM THE NORTHWEST
Ph. Geoffrey Alington

PLATE II



A, ROMAN FORTLET, DALMAETHAR (*see p. 282*)
Ph. Geoffrey Alington



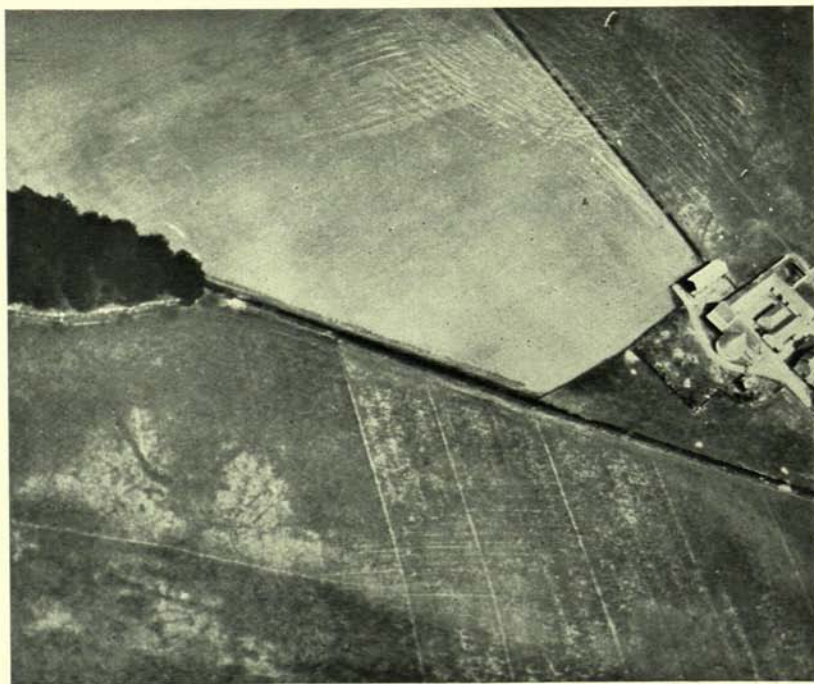
B, ROMAN FORTLET, REDSHAW BURN (*see pp. 281, 282*)
The Roman road is the upper one, forking from the 18th cent. road
Ph. Geoffrey Alington

PLATE III



A, ROMAN ROAD NORTH OF BUSHEL BECK NEAR MOFFAT, SHOWING QUARRY-PITS (see p. 283)

Ph. Geoffrey Alington



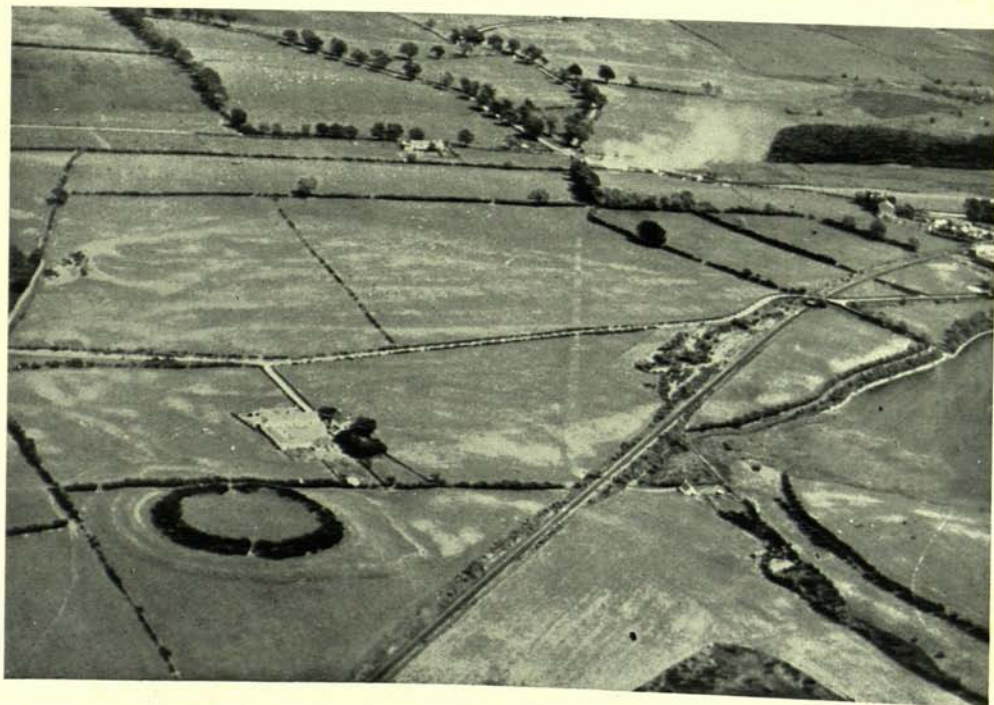
B, SOUTHWEST CORNER OF ROMAN MARCHING-CAMP, TORWOOD, WEST OF
LOCKERBIE, REVEALED BY PARCHING (see p. 284)

Ph. Geoffrey Alington

PLATE IV



A, NATIVE FORT, WARD LAW, CAERLAVEROCK, DUMFRIES, IN TREES; BEYOND, A PROBABLE ROMAN (SQUARE) FORT FAINTLY VISIBLE (see p. 284)
Ph. Geoffrey Alington



B, ROMAN ROAD NORTH OF LOCHMABEN REVEALED BY PARCHING (WHITE LINE=BROWN ON GROUND)
 • ROUND NATIVE FORT OF WOODY CASTLE IN FOREGROUND (see p. 284)
Ph. Geoffrey Alington

PLATE V

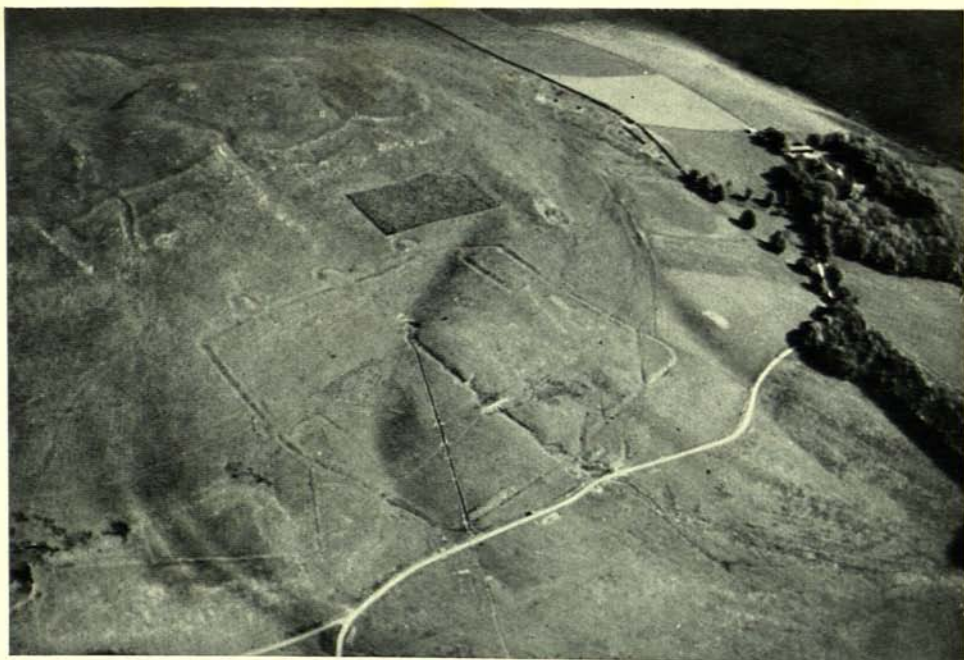


A, ROMAN MARCHING-CAMP (?) AND NATIVE PROMONTORY-FORT (?) GALLABERRY, NORTH OF CARZIELD, DUMFRIES; REVEALED BY PARCHING (see p. 285)
Ph. Geoffrey Alington

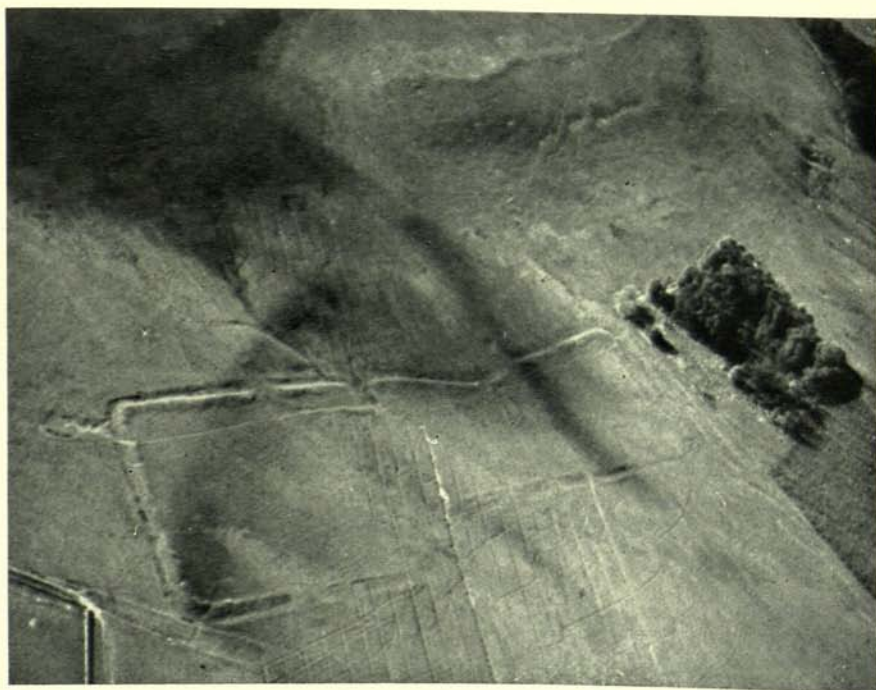


B, ROMAN FORT, BIRRENS, WITH ANOTHER FORT, AS CROP-SITE IN FOREGROUND (see pp. 285-6)
Ph. Geoffrey Alington

PLATE VI



A, ROMAN SIEGE-FORT ON SOUTHEAST SLOPE OF BURNSWARK, DUMFRIESSHIRE (*see p. 286*)
Ph. Geoffrey Alington



B, ROMAN SIEGE-FORT ON NORTHWEST SLOPE OF BURNSWARK (*see p. 286*)
Ph. Geoffrey Alington

PLATE VII



A, MEDIEVAL STRIP-LYNCHETS ON SHAW HILL, ABOVE CULTER, LANARKSHIRE (see pp. 289, 290)
Ph. Geoffrey Alington



B, MEDIEVAL STRIP-LYNCHETS ON THE EAST SLOPE OF ARTHUR'S SEAT,
 ABOVE DUNSAPIE LOCK, EDINBURGH:
 Note the corresponding group of riggs and furrows immediately below (see pp. 289, 290)
Ph. Geoffrey Alington

AIR RECONNAISSANCE OF ROMAN SCOTLAND

and took all the photographs. Without his cooperation the results here described could not have been obtained.

We left Southampton at 11.15 a.m. on 6 June 1939, and landed on Lanark race-course at a quarter to four. The weather was perfect, and visibility excellent except where obscured by the smoke haze of the midlands. Soon after leaving Southampton I observed on the horizon ahead of us a plume of high cirrus cloud. I watched it gradually get higher; but it was not overhead until we were well north of the latitude of Liverpool. When first seen, therefore, it must have been about 180 miles distant. (We were flying at about 2000 feet or lower). We flew along the Lancashire coast and over the Cumberland mountains, crossing the Solway to Annan, where we reached familiar ground. From here up Annandale and down the Clyde we followed the Roman road and made the first discoveries. I observed a small unrecorded native fort on Woodcock Air (Dumfr. 57 S.E.), in a clearing on the top of the hill, and another on the top of Quhytewoolen Hill (Dumfr. 43 S.W.) where I had already expected to find one. I saw another on Quothquan Law (Lan. 33 S.E.), divided into two parts by an earthen rampart; from the ground I had suspected the existence of such a fort, but from some points of view the hill-top seemed quite devoid of any fortification. (One of the advantages of air reconnaissance is that it avoids the time and labour of climbing likely-looking hills that may prove barren of remains at the top!). After our arrival at Lanark we decided to go up again and confirm these discoveries. The evening was cloudless and the brilliant low light perfect for observation. We flew back to the Devil's Beeftub above Moffat and then up the Potrail Water to Durisdeer, confirming the existence of two Roman fortlets discovered on the way up and finding another probable one. These were at Bushel Beck (Dumfries 9 S.W.) and Redshaw Burn (Lanark 51 N.W.; PLATE II B); the doubtful one was beside the Durisdeer road at Allan Cleuch (Lanark 53 S.W.). I also observed the course of the Roman roads, confirming my field-work everywhere, and adding a portion north of Crawford.

These fortlets are a recent addition to knowledge, at any rate as a normal accompaniment of Roman roads. They are partly due to air observation, one of them being that at Barrock Fell (Cumberland 31 S.W.), discovered and photographed from the air by Group Captain Insall, V.C., in 1930. This was excavated by Professor R. G. Collingwood and Mr R. E. Porter (*Trans. Cumb. and Westmorland Arch. Soc.*, 1931, N.S. XXXI, 111-18). The existence of such fortlets has of course been suspected since the time of General Roy, whose site at Tassiesholm

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belongs to this group. Last year another was identified at Dalmakethar (Dumf. 33 NE, PLATE II A) by Mr K. St. Joseph in the course of his field-work done, in a voluntary capacity, for the Ordnance Survey, while surveying the Roman roads and sites on the Border Sheet; and the fort at Durisdeer, for which a Roman origin had often been claimed, was proved to be in fact Roman by Mr J. Clarke's excavations.

The redoubt in the NW corner of the Roman fort on Burnswark (SE slope, PLATE VI A) is another of the same kind. Later on we found yet another near the Beeftub, at Rowantree Grains. There are thus five examples of this type of fortlet along the Roman road between the marching-camps at Little Clyde and Torwood Muir. All are placed close beside the Roman road, with the single entrance facing it. It is not yet possible to say whether they were placed at any stated intervals; Redshaw and Rowantree are less than three-quarters of a mile, and the others several miles, apart. But there may be many more to be discovered even on this road; and it seems reasonably certain that they will be found on other roads within the military area of Roman Britain, and perhaps elsewhere. The one thing that can be said with certainty is that some of them are placed at the crossing-points of ravines, probably to guard a wooden bridge. Their shape is shown best by Redshaw and Dalmakethar (PLATE II A); it is that of a small rectangle with rounded corners, or of a flattened oval. In proportion to the area enclosed the ramparts are rather wide, and that is one of the features which enables them to be recognized when seen from above. They have, moreover, when thus seen, the massive regularity characteristic of all Roman work. These features enable one to distinguish Roman fortlets from native works, whether prehistoric, medieval or later. Several of the native forts, particularly on the hills above Annandale, closely resemble these Roman works, and may have been influenced by them. One such on Broomhill bank, for instance, is less than two miles to the E.S.E. of Dalmakethar.

We flew several times along the Roman road through Annandale and the Upper Clyde, and each time we made fresh discoveries. On one occasion we found a small round signal station of the Gask type at the top of the pass near the Beeftub. It consisted of a ditch with a break (for entrance) facing the road. We thought we saw remains of others along the ridge further south (and elsewhere), but these must be tested on the ground. It is easy to confuse these slight relics with the remains of sheep folds; but the latter consist of a rather broad round bank, usually without any break in the circumference.

AIR RECONNAISSANCE OF ROMAN SCOTLAND

The stretch of Roman road from the Beeftub Pass to Moffat golf-links (5 miles) is one of the best and most perfect in the kingdom. The ground over which it passes is moorland that has never been cultivated. There are places, especially at the northern end, where it is almost lost in bog; but there are no other considerable breaks in its continuity. Its Roman origin was never seriously questioned by the earlier field archaeologists; and their opinion was confirmed by me when I walked along it in 1924, and restored it as such to the one-inch Ordnance Map. Since then I have walked along it again throughout its whole length from its northern known limit* to Burnswark. The quarry-pits from which materials for the causeway were dug accompany it on either side throughout, and are well seen on PLATE III A. These quarry pits are in fact valuable aids to recovering the course of the road, when the causeway itself is not easily traceable. (I was thus put on the right track by a row of quarry-pits on the hill above Walstone, N.E. of Carlops; air-observation revealed the quarry-pits very clearly, together with the causeway between them, which was barely visible on the ground). The later (18th century) road-makers followed a different procedure. They obtained the materials for their road-causeways not from small pits dug in the flat ground beside the road but from much larger quarries dug in the side of the hill where the road ran as a shelf along the slope. These later hillside quarries accompany the 18th century successor of the Roman road and medieval track over the same route by the Beeftub. Here they are fairly regularly spaced at intervals of about half a mile. The presence of such hillside quarries beside a causeway road is a warning that the road in question is not, in its present form, at any rate, to be accepted as Roman.

* North of Little Clyde Mr St. Joseph traced its course in 1938 through Upper Clydesdale to Crawford. Thence it goes over the Raggengill pass to join the Clyde valley again near Coldchapel. At Wandel it falls into the modern road with which it seems to coincide to Candeyburn, north of Biggar. There is a perfect stretch of it south of Melbourne, on the moor east of the modern road. I found a mile of it between Dolphinton and Hardgatehead from the air. There is another well-preserved section north of West Linton. In both these last instances it is plainly distinct from the metalled (18th century?) road which runs more or less parallel to it at a considerable distance. It is last seen for certain, with quarry-pits, on the hill above Walstone. I discovered it last winter. Mr St. Joseph has referred me to Maitland (1, 193) who says it reached the 'eastern end of the Pentland Hills, in the neighbourhood of which it is to be seen pointing to the station at Cramund'. The Annandale road thus ran towards Edinburgh rather than Glasgow, as has previously been supposed, and never went to the fort at Corbiehall near Carstairs; though of course there may have been a branch thither separating off perhaps near Biggar.

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Later on we reconnoitred some other Roman sites in the neighbourhood of Lockerbie and Dumfries. We saw the missing sw corner of the marching-camp at Torwood (PLATE III B) which eluded Roy. It appeared as a bright green mark, like an L with round corner, in a field that had been parched brown by the drought. Recently both Mr St. Joseph and I had found the *tutuli* guarding gates in its west side, now hidden in the wood. Suitable conditions would doubtless reveal the ditches of its other sides, which pass through fields now in cultivation.

Earlier in the year I had visited the site of a reputed Roman fort at Ward Law (PLATE IV A), above the Castle of Caerlaverock, 7 miles south of Dumfries. This was seen in 1772 by Pennant, who described it as 'faint vestiges of a Roman camp, now much ploughed up' (Tour I, 1790, 108: he is wrong in saying that it lay on the *south* side of the hill). The mound was still sufficiently plain last winter for me to be able to trace it all round, and even to photograph it on the ground. Its outline, fortunately, was marked by a broken line on the Ordnance Map in 1856, when doubtless it was more evident than now. It is nearly square in plan and has the appearance of Roman work; but it cannot be marked as Roman on our map until it has been tested by excavation. When flying over it I could hardly see it, for the crops, if any, must be unsuitable for showing up the ditch; but on the photograph the ghostly outline is faintly visible above the clump (which hides a native fort). The wall running north from the clump divides it approximately in half. Round the outside of the clump on the west and south runs a belt of bright green grass revealing the outermost ditch of the native fort; and south from it is a long row of tiny black (=green) dots representing pits, probably post-holes.

There are persistent accounts in the old writers of a Roman road running westwards north of Lochmaben by Woody Castle. It is marked as an 'Old Road' on the Ordnance Maps. I had already investigated this on the ground during the winter, and carried it a little further west; it seemed to be Roman, but I was not prepared for so startling a confirmation as that which I now saw, and which is shown on PLATE IV B. The straight white band pointing directly at the observer is a far more authentic record than the almost imperceptible fold which was all I could see on the ground. The photograph proves, therefore, not so much the value of air observation (though it does this) as the reliability of careful ground observation, even when the visible remains are of the slightest. The road continues along a curiously zigzag course westwards to Broomrig; but, though we got a hint or two of

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its possible continuation (to be tested on the ground later), all our efforts to trace it definitely were unsuccessful. The hints we got were too slight for mention, but they are consistent with the old accounts which took it by Murder Loch. Whatever its exact route the road must surely have been making for Carzield or its neighbourhood; for at Carzield a Roman fort has recently been excavated by Mr I. A. Richmond and Mr Eric Birley. We had a look at it, and saw the western ditch of the fort, invisible on the ground, plainly revealed as a broad green belt along the steep bank of the plateau. But we could see no remains of the road, as we had hoped, issuing from it, except possibly two dark green curved lines on the eastern slope outside the fort, pointing towards the modern road from Carzield to Kirkton. On the other hand we did discover, close by at Gallaberry, a rectangular enclosure with rounded corners which can hardly be anything but a small Roman marching-camp (PLATE V A). The ditch was revealed by a bright green band contrasting vividly with the toast-coloured grass elsewhere in the field. The region is one of glacial moraines, forming a network of hummocks and hollows, often containing lakelets or bogs; and the tops of the hummocks, and the gravel river-terraces, react strongly to drought. The grass is burnt brown, and any former pits or ditches, having more depth of soil, remain moister and keep their verdure. This sort of country forms an excellent hunting-ground for the air observer during periods of drought. Had we had more time to spare we could certainly have found many more sites. As it was we found two large circles in the bend of the road leading to Brownfield (immediately south of the farm and west of the road), a mile and a half SE of Carzield.

The Roman camp, if such it is, stands on a gravel plateau projecting into the marshy low-lands of the Nith. Across the neck of the promontory runs a curved green line (= a ditch), representing probably a small native fortlet. Close to it, abutting on one end, is a small three-sided enclosure of an unknown character. Along the foot of the bank, in front of the only entrance to the Roman camp, is a long green line extending beyond the width of the camp at either end. Other lines are probably of later (agricultural?) origin; and a small circle in the foreground, with a gap flanked by two post holes (?) may be a hut enclosure. It seems to stand inside another much larger circle.

The two most celebrated Roman sites in this neighbourhood are Birrens and Burnswark. Birrens has long been recognized as a Roman fort and was excavated in 1895 and 1936-7. It is most interesting to find the outline of another work close beside the well-known fort

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(PLATE V B), and on a slightly different orientation. This is undoubtedly the work planned by Roy (pl. xxiv), but the apparent impingement of the two adjacent sides suggests that it cannot be an annexe, as might appear from his plan. One may conjecture that this is the Flavian fort, for the earliest structures on the other site, definitely recognized to belong to a Roman fort, were apparently Hadrianic in age. The three ditches of the new fort are plainly seen from the air, outlined by yellow flowers, which appear as parallel white bands on the photograph. No certain traces of a road issuing from the fort were seen, nor was it found, in spite of constant search, between here and West Gill beyond which to Burnswark its course is known. But the search produced a small native fort, seen as a confused crop-site, midway between Woodlands and Langdyke.

Burnswark is a flat-topped hill 3 miles NW of Birrens. It is a landmark for all the country round; we could see it from the aerodrome at Carlisle, and I have seen it from the hills near Crawford. From the air it can be seen from even further north, and on our return from Edinburgh on 12 June we saw it from near Biggar and flew straight across country towards it. At close quarters it has a certain grimness that is in keeping with its history¹; for it was the 'tribal capital' of the natives who were besieged there by the Romans; and near it was fought the celebrated battle of Brunanburh (see ANTIQUITY 1937, XI, 283-93). The siege-camps of the Romans are still very well preserved. That on the southeastern slope of the hill is the more perfect of the two (PLATE VI A). It has been suggested that the camp on the NW was unfinished. Our illustration (PLATE VI B), shows however that all four sides were finished. On the south side of the hill, close to the Roman road, is an irregularly shaped earthwork, which has been regarded by some as a Roman redoubt. I have examined it on the ground, and Alington photographed it from the air. Its age and character are still rather uncertain. I am inclined to connect it with the hill-fort on the summit, for there are traces of a track leading apparently from it up the hill. On the other hand, there are the foundations of a rectangular building (black house?) in a small enclosure between it and the Roman road, with a small cultivation-plot (rigg and furrow), surrounded by a bank, attached.

¹ The bare austerity of its outline is not improved by recent planting on the SE slope, close to the corner of the Roman siege-camp.

AIR RECONNAISSANCE OF ROMAN SCOTLAND

Elated by our discoveries in Annandale we decided on the second day of our expedition to fly northwards and see what we could make of the Roman remains in Perthshire and Angus. The region lay rather far from our base, from which it was separated by the Gargunnock Hills, the Ochils and the Sidlaws ; and there are times when these uplands can become unpleasant obstacles to flying. Consequently I was anxious to get this distant area worked off quickly while the weather was settled. We flew first to Perth, where we filled up, thence along Strathmore to Aberdeen. Thence we returned in one flight by Comrie, Glen Artney and Callander. During the flight we inspected all the known Roman forts north of the Antonine Wall except Carpow, and found another further north than any yet known.

First of all I wanted to look at Myot Hill near Denny, which had been connected with the Mæatae². Sure enough on its top I saw the plain outlines of a native fort, hitherto unrecorded. At Stirling we examined and photographed the royal garden called the King's Knot, below the Castle on the west. Here Maitland³ says a 'Roman station' was destroyed when the gardens were being laid out. The statement seemed very probable, and I had already wondered whether the Knot itself might not have been much older than the gardens, and had marked on my map a ditch which seemed to have no connexion with them. From the air we saw this ditch and another, certainly older than the gardens, which may be part of the remains aforesaid ; but the two ditches seem to have no connexion with each other. It is as certain as anything can be that there was a Roman fort or fortlet to guard the Forth crossing, and this is the obvious place for it. All the earlier accounts, and the existing topography, make the Roman road to Ardoch pass close by, to the west of the Castle Hill.

This is not the place to describe our observations in detail. By far the most important discovery was in part accidental. Years ago I had ringed a site at Cardean near Meikle on my map for investigation, but had forgotten why, except that Roman remains had been found there. When we were flying over it I was just saying to Alington that we had better go on and not circle it, as there was nothing, when we both saw something coming into view. It is a curious fact that even 'flat' sites, like crop sites, are invisible or barely visible from certain angles.

² Watson, *Celtic Place-names of Scotland*, 59.

³ *History of Scotland*, 1757, I, 194.

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What we saw was the broad, unmistakeable rampart of a Roman fort, complete with internal streets, gateways and road leading northeastwards out of the eastern gate ! Though so plainly visible the site was not an easy one to photograph, and the only one taken is not suitable for reproduction. (It shows the features described, but not plainly enough to appear in a half-tone reproduction). The photograph has, however, been seen and passed by qualified students of Roman Britain ; nor have I the slightest doubt about the character of the site.

The implications of this discovery are far-reaching. It means that at least one other permanent Roman fort is to be looked for northeast of Cardean ; that the Romans extended their permanent occupation of Scotland at least half way up Strathmore. If we eliminate the shadowy Constantius, that advance can only have been made by Agricola or Severus. Agricolan remains have been found at Inchtuthill 11 miles sw of Cardean. A Roman road has been traced at Westmuir pointing towards Kirriemuir (it is marked on the map of Roman Britain). Other forts should be looked for somewhere in the neighbourhood of the marching-camps north of Forfar and Brechin respectively. There may be another near the marching-camp at Kirkbuddo, where a Roman road has been recorded.⁴ Such a road if it existed must surely have connected a fort on the Strathmore road with a port on the coast, at Arbroath or Carnoustie perhaps.

Beyond Strathmore a line of Roman advance is indicated by the Roman marching-camps at Raedykes, Peterculter and Glenmailen. Between the last two is an alleged marching-camp near Kintore. There is also an alleged fragment of Roman road at Inverurie, but this should be regarded with a good deal of suspicion. Claims have been made for sites further north even than Glenmailen. All these should now be examined by modern methods. The route indicated is the natural route of advance to the north. Whether any permanent Roman forts will be found so far north is doubtful. The line indicated by the marching-camps reaches the sea at or near Stonehaven and then turns inland again. Somewhere here, one imagines, contact was made with the fleet, and a base established for a fresh advance. One might expect therefore to find a permanent fort at Stonehaven and two between there and Cardean.

It is perhaps significant that Cardean should be close to Meikle

⁴ Maitland, *Hist. of Scotland*, 1757, I, 200. For the name and its associations see Watson, *Celtic Place-names of Scotland*, 313.

AIR RECONNAISSANCE OF ROMAN SCOTLAND

where so many sculptured stones have been found (and where they are still to be seen in the local repository). If the place was an important centre in the Dark Ages, as it clearly was, it was probably no less important earlier. Other Dark Age centres, as indicated by sculptured stones, were at St. Vigeans near Arbroath, Dunnottar near Stonehaven, and Inverurie.

It is high time, however, to curb these flights of the imagination and resume the narrative. The chief object on our return journey was to look for a Roman road up Glen Artney connecting the Roman forts near Comrie with Callander, where another fort is demanded by the topography. We failed to find any trace whatever of such a road, although much of the route consists of open moorland where it would surely be visible if it had ever existed. We had a look at Bochart, where I had gone to look for a Roman fort in the winter, but could see no hint of anything there or in the immediate neighbourhood, though crop conditions were favourable. We turned to fly home over the Vale of Menteith and the desolate moors of Gargunnock, with a view extending from the Firth of Forth on the east to the sea and Arran in the west.

Although we had concentrated our attention upon Roman roads and sites, we had tried not to neglect entirely the remains of other periods. Native forts were so numerous that it was difficult, and sometimes impossible, to check them upon the map. We got a good bunch, as crop-sites, in the Lothians, indicating that this fertile lowland was once thickly covered with them. It is certain that if anyone could carry out a systematic air survey of this area, he would be well rewarded; but such a survey would be useless except in May or June. It should, moreover, be carried out in a series of successive years, because only when the crop grown is corn do the sites appear as a rule. (Roots, clover, grass and potatoes are not responsive to antiquity). We were also able to observe the strip-lynchets which have attracted the attention of Scottish archaeologists from the days of Gordon. I have been observing them myself for many years and I have now come to the conclusion that there are two quite distinct types: (1) the irregular scattered shelves, such as those on the west bank of the Clyde near Elvanfoot and between Abington and Crawford; (2) the orderly series which occur in parallel rows on Shaw Hill, Culter (PLATE VII A), on the east side of Arthur's Seat, Edinburgh (PLATE VII B), and at Dunsyre, Robertson, Romanno and Old Thornielee. Those of the second class can clearly

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be seen (in both our illustrations, particularly VII B),⁵ both from the air and from suitable view-points on the ground, to belong to the same system as a series of riggs and furrows which abut upon them at right angles. The strip-lynchets are in fact, as I have always maintained, merely the form which the strips assume when laid out horizontally along a steep slope. I cannot conceive that any reasonable person looking at any of the sites mentioned impartially should be able to deny that both are parts of the same field-system, and were cultivated contemporaneously. Whether Type 2 developed out of Type 1 is another matter; but it well may have done so. Type 2 is plainly not earlier in date than the Middle Ages, and may have lasted down to the 17th century or later. The rigg and furrow system is exceedingly common all over the lowlands. I have recently seen good examples of it in the parish of Shotts, where it is associated with derelict farms. It is probable that some of the mysterious groups of earthworks, of which we saw an embarrassing number during our flights, consisting of rectangular and round enclosures, are in reality the remains of medieval farms and sheep-folds. They are often associated with fields of this type.

We were unable to complete the whole programme I had mapped out; but that is the normal fate of programmes. I had hoped to fly along a new Roman road into Ayrshire recently established by Mr St. Joseph, and perhaps to find some fortlets along it. I had also hoped to investigate a possible line of fortlets along the coast of Renfrew and Ayr, and to find a Roman road from Irvine to Glasgow. But these projects can well be postponed until the way has been prepared by field-work. We have at any rate profited greatly by the experience, and added a number of new sites to the map. It is evident that concentration and specialization are desirable to produce the best results; that the time and season are all important; and that air observation is most profitable when one has previously familiarized oneself with the topography by fieldwork. It is also clear that one can often classify earthworks into native, Roman and (late ?) medieval simply by looking at them from above; after all it is only thus that one can really see them as a whole at all. Thus seen, one can recognize the different types just as if they were pots or axes.

The exploration of ancient Scotland from the air has only begun. It is one of the most promising fields of research anywhere in the world.

⁵ The rigg and furrow system at Culter is visible on the original photograph, but is obscured by a cloud-shadow. I observed this carefully at the time with this very point in mind, and satisfied myself about it.

AIR RECONNAISSANCE OF ROMAN SCOTLAND

NOTE I

The Roman fort at Cardean is not a completely new discovery ; it is rather the authentication by modern methods of a site that has long been on record. No one would have ventured to assert, on existing evidence, that it was of this character. The revelation of the rampart and road by air observation and photography now makes it possible to state with complete certainty that it is the site of a Roman fort, and further that a Roman road ran NE from the gate in its north-eastern rampart. Sir George Macdonald has kindly sent me a quotation from a holograph diary of a journey made by Roy's friend, General Melville—the discoverer of the Strathmore camps—in 1777. It is plain that General Melville saw the Cardean fort, but there is nothing in his description to suggest that he recognized it as Roman. The *New Statistical Account* (Forfar, 1845, 679) describes it as a Roman Camp which, though 'partially obliterated by agricultural operations, can still be distinctly traced'. Major Bayley, R.E., in 1861, was still 'able to identify the extent of the camp'. On 11 July 1925 I visited the site and could find no surface indications of it at all.

The Ordnance Map (Forfar 42 NE, 43 NW) records certain discoveries here, about which fuller details are entered in the original Object Name-book of 1861, compiled at the time of the first large-scale survey. The 'earthen vessels, etc., found' along the edge of the hill to the NW within the area of the fort, are described there as consisting of 'earthen vessels, human remains, iron implements and coins'; and 'other articles' were also found, at various times. The authorities for these discoveries were Mr H. J. Fitchie, of Deanfield, and David Gibb, Forester. Mr Fitchie in a letter to Major Bayley corroborated David Gibb's statement. Midway between the east gate of the fort and the farm of Wester Cardean a 'Roman sword' was found. The site of its discovery was pointed out to Major Bayley by Admiral Popham of Cardean and David Gibb ; but it seems to be approximate only, as in the Name-book it is said to have been found 'hereabouts'. Major Bayley describes it as a 'straight iron sword' pronounced to be Roman by Lord Talbot de Malahide in whose possession it then was. There seems no reason to doubt this opinion ; but it would be interesting to know what has become of it. Close by is the site of a 'weem' or earth-house, 'of which some of the stones indicating the site were uncovered' in Major Bayley's presence. I could find no trace of it in 1925, but did not make any local enquiries.

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The Roman road, too, has quite a respectable pedigree. The Ordnance Maps mark it only at Westmuir, 6 miles NE of Cardean and $1\frac{1}{2}$ miles SW of Kirriemuir. Maitland (*Hist. of Scotland*, 1757, I, 200) refers to it at Westmuir ('Riddy').⁶ The *New Stat. Account* says definitely that there was a Roman road running east from the camp at Cardean along Strathmore. In 1861 the Ordnance surveyor described the fragment at Reddie (Westmuir) as 'a small portion of an old road raised about $1\frac{1}{2}$ feet above the level of the adjacent ground'. Major Bayley says that the 'whole of the distance here mentioned' (*i.e.* in the *N.S.A.*) has been more or less identified; but it is not clear whether he is referring to the whole course from Cardean to Westmuir or merely to the 500 yards then visible at Westmuir; probably the latter, for otherwise he would surely have inserted its course on the Ordnance map.

I wish here most gratefully to acknowledge the courtesy of the Glasgow Archaeological Society in allowing this preliminary notice to precede its own much more substantial publication of the results, and to thank Mr St. Joseph for his valuable cooperation. For the statements in this article, however, I alone must be held responsible.

⁶ Roy investigated it in 1771 (M.A. 108).

Pliska*

by IWAN WELKOW

Director of the National Museum, Sofia

WEST of the Provadia hills in Bulgaria is the valley of Shumen. The northern part of this valley belongs to the basin of the Provadia river, the southern to that of the river Kamtchia. The present railway line from Shumen to Kaspitchan serves as an ideal boundary between these two regions, but the two valleys are best known because of two historical sites, Preslav and Aboba (now Pliska).

The rivers of the Aboba plain have very little water during the summer season, but during winter and in time of heavy rains they overflow and cover great parts of the barren valley. The winter there is cold and severe, the summer dry and hot. In the plain, north of Kaspitchan railway station, are the ruins of Aboba. Eshberüch or Ispërîch (Asparuch), the younger son of Kurt (644-702), in his march southwards stopped there, and founded the first capital of the Bulgarian khans, later to become the magnificent Pliska.

For centuries the ruins of Pliska were almost completely forgotten. None of the medieval travellers mentions them, nor do any of our chroniclers. The roads changed their course southwards and in time even the name of the old fortress was forgotten. Early in the 19th century the name of Aboba first begins to be mentioned again, when its ruins attracted the attention of travellers. Niebuhr, in his *Reisebeschreibung nach Arabien* (Hamburg, 1837) spoke of traces of an old settlement in the vicinity of Aboba and Novi-Pasar. Later Kanitz drew attention to the ruins of Aboba. In his book *Donau-Bulgarien und der Balkan* (1882, vol. III, 254) he wrote: 'Following the directions which were given to us, half an hour from Aboba I found a large plain full of ruins with the towers and fortress-walls of a well-preserved Roman castle, of whose existence there was no previous record'. Basing his observations on the inscriptions found there, he assumed it

*Translated from the Bulgarian by Dr Ivanka Akrabova. Published by 'Bulgarian Starina', Shumen, and printed at the Court Printing Press, Sofia, 1930.

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to be Burdisus, but immediately afterwards threw doubt on his assumption, because Burdisus Bulgarophigon, the present city of Baba-Eski, is to be found between Adrianople and Constantinople. He also says 'if so large and so well preserved a castle were ever to be discovered in our country, it would draw general attention, and many archaeological and historical monographs would soon be written about it'.

Ireček passed through the same places a few years after Kanitz. He had better opportunities of observing the ruins well. Ireček too, at first thought that they were Roman, but identified them with Abrittus in Misia, which, as Škorpil proved, is to be located near Abtaat in the Dobrudja. Ireček also thought they might be Pliska, mentioned by the Byzantine writers Leo Diaconus and Anna Comnena.

Karl Škorpil was the first to point out that the earliest capital of the Bulgarian khans should be looked for near the present Aboba. Although he did not bring forward much evidence in support of his opinion, it seemed so probable that it did not meet with any opposition. Even Ireček declared later that the old Bulgarian capital must not be looked for near Preslav, because its geographical situation is not suitable for a cavalry camp, but rather at Pliska. There, according to him, was the main camp, or 'Hring', of the proto-Bulgarians, whose horses, according to the testimony of Masudi, grazed freely over the meadows, ready for a sudden march.

The excavations of the Russian Archaeological Institute began at Pliska in October, 1889. The first object was to excavate the central mound where, according to the local peasants, a great fortress once existed. In 1900 the excavations were resumed in the vicinity.

The camp of the proto-Bulgarians had been there even in the eighth century, when the earthworks were thrown up. For in the plain of Aboba there are two kinds of defensive constructions, of earth and of stone. Some of the earthworks are very well preserved, but the majority have disappeared completely, especially in cultivated regions. There are earthworks also in Novi-Pasar, but they are of Turkish date.

The Great Earthen Rampart forms an irregular quadrangle; the north side is 3.9 km. long; the west and the east sides are nearly the same (7 km.) length; the southern side is 2.7 km. At present it is impossible to determine the exact number of original entrances. Only where remains of made roads can still be found can we presume that there have been entrances of some kind. There were two roads going northwards, one eastwards and one westwards. In the southern part there was an entrance near Assar Dere. (FIG. 2, p. 297).

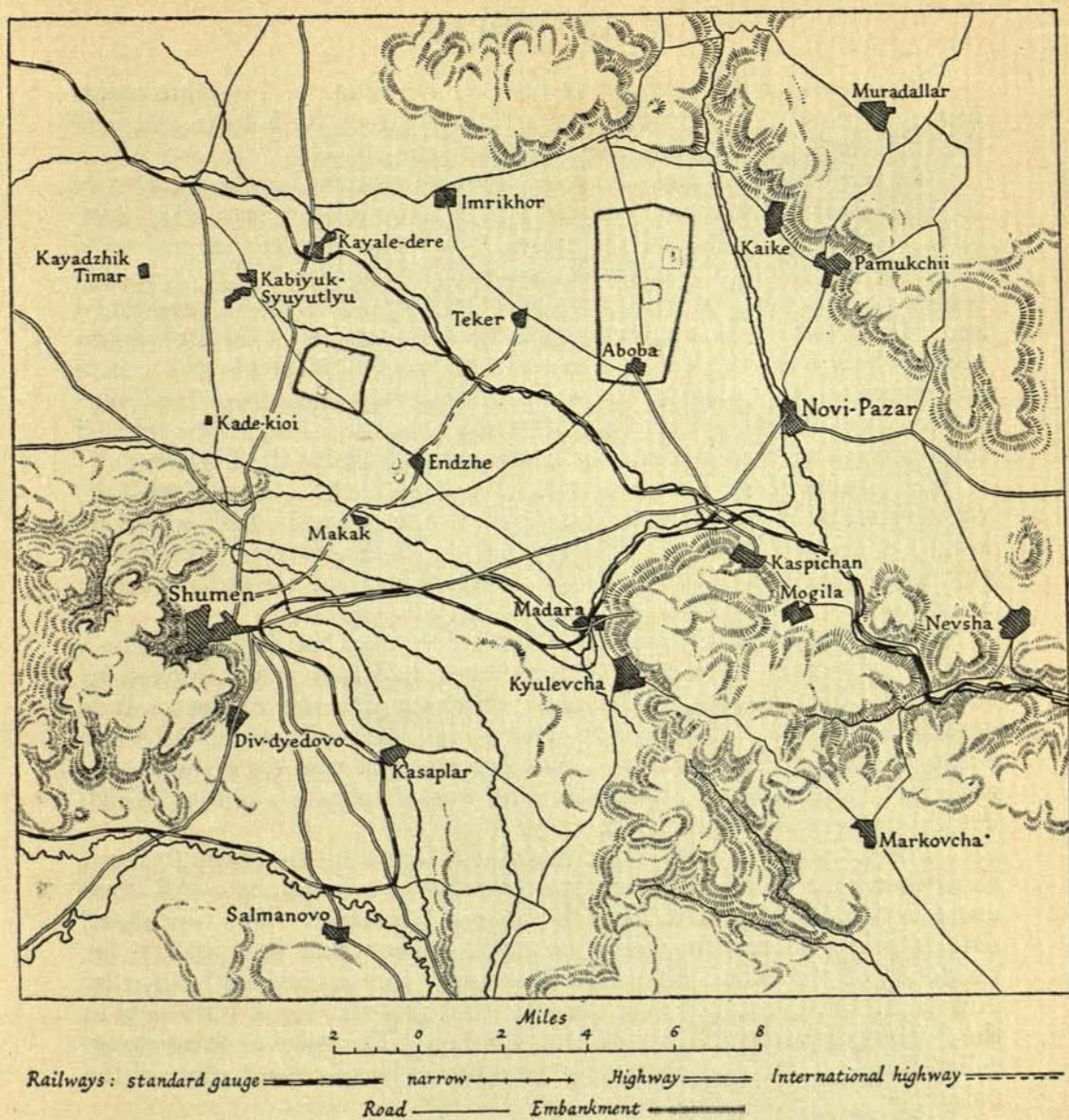


FIG. 1. THE VALLEY OF SHUMEN AND THE SITE OF ABOBA (NOW PLISKA), BULGARIA

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The bank of the rampart is on the inner side. Its measurements vary according to the terrain. It is 12 m. wide at the bottom and near the surface, 3.5 m.

In the centre of these huge earthworks, which enclose an area of no less than 23 square kilometres (9 square miles), is a stone fort, built at the beginning of the ninth century. In the inscription of Omortag, as also in an apocrypha speaking of the vision of the prophet Isaiah and in the Byzantine writers, this stone fortress is mentioned and called Pliska. It is known that Bolgar, the capital of the Bulgarians settled along the river Volga, was also surrounded by earthworks; there still exists a linear earthwork, 400 km. long, extending from the Volga to the river Kama. This type of construction the Bulgarians learned from the Huns, who in their turn were influenced by the Chinese.

The ruins of the Pliska fortress have the form of a trapeze with the western and the eastern walls parallel. The western wall is 788 m. long, the eastern, 612 m. The southern and the northern are almost equal, 740 m. each. The walls are 2.6 m. thick, and are built of large, ashlar blocks, evenly laid.

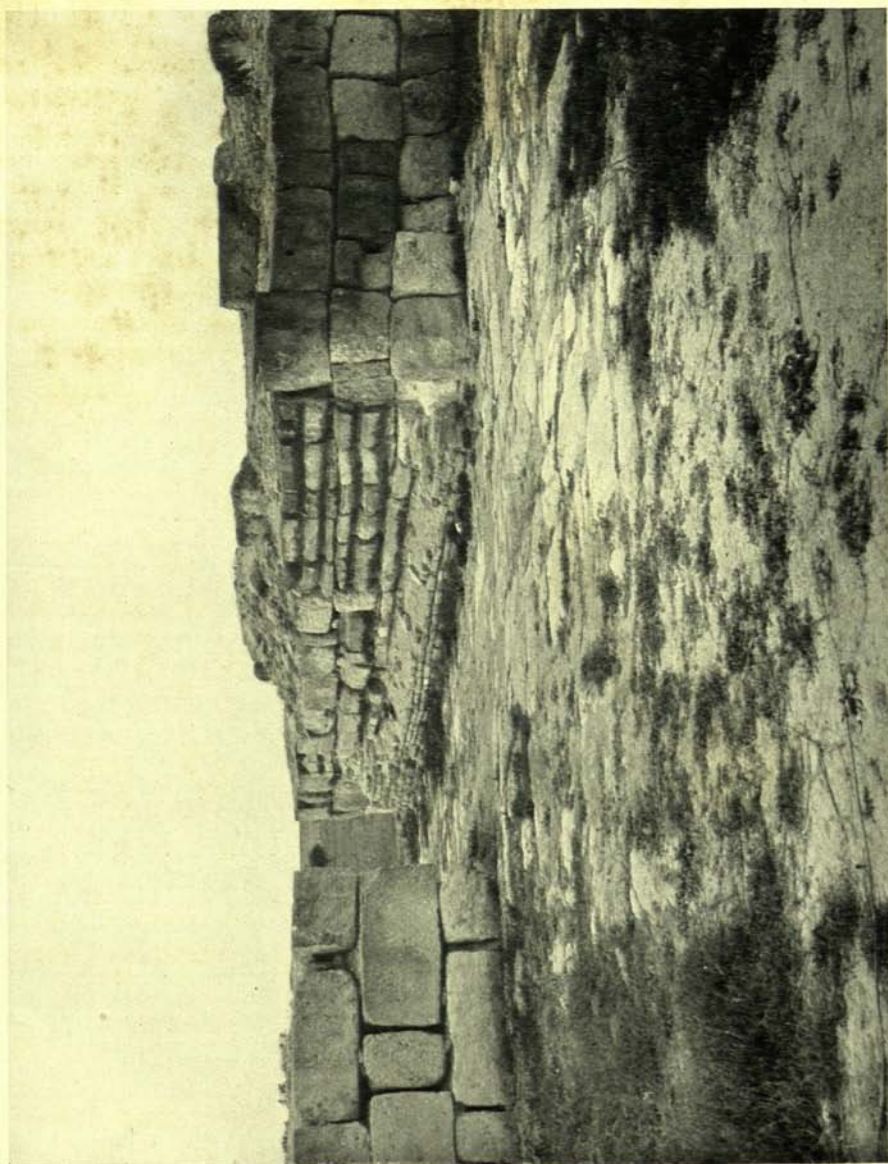
There was a round bastion at each corner of the fortress, with a pentagonal one on each side. The fortress had four gates, flanked by square double bastions. The main entrance was the eastern. It is assumed that the walls were 10-12 m. high, the only access to them being by way of the bastions. The eastern gate was 4.7 m. wide and probably vaulted with bricks. There were two gates, one of which could be lowered (PLATES I, II).

The remains of great stone buildings, found in the inner fortress, show that here we are dealing with the ruins of a large city. All these constructions seem to date from the time of Omortag. This postulated date acquires some probability from the known fact that in 811 the residence of the Khan Krum at Pliska was surrounded and burnt by Nicephor. It must, however, be admitted that the stone buildings in the fortress date from the period after the fire. The ruins of this fortress called Kale-eri, are also known by the name of Sarai-eri, *e.g.* place of the palace.

The largest and the most important of the ruins are those of the Great Palace or Throne-room. This was a monumental construction. It had the form of a rectangle 52 by 26.5 m.; the outer walls are 2.2 m. to 2.6 m. thick; but the southern wall is less, 1.6 m. (PLATE III).

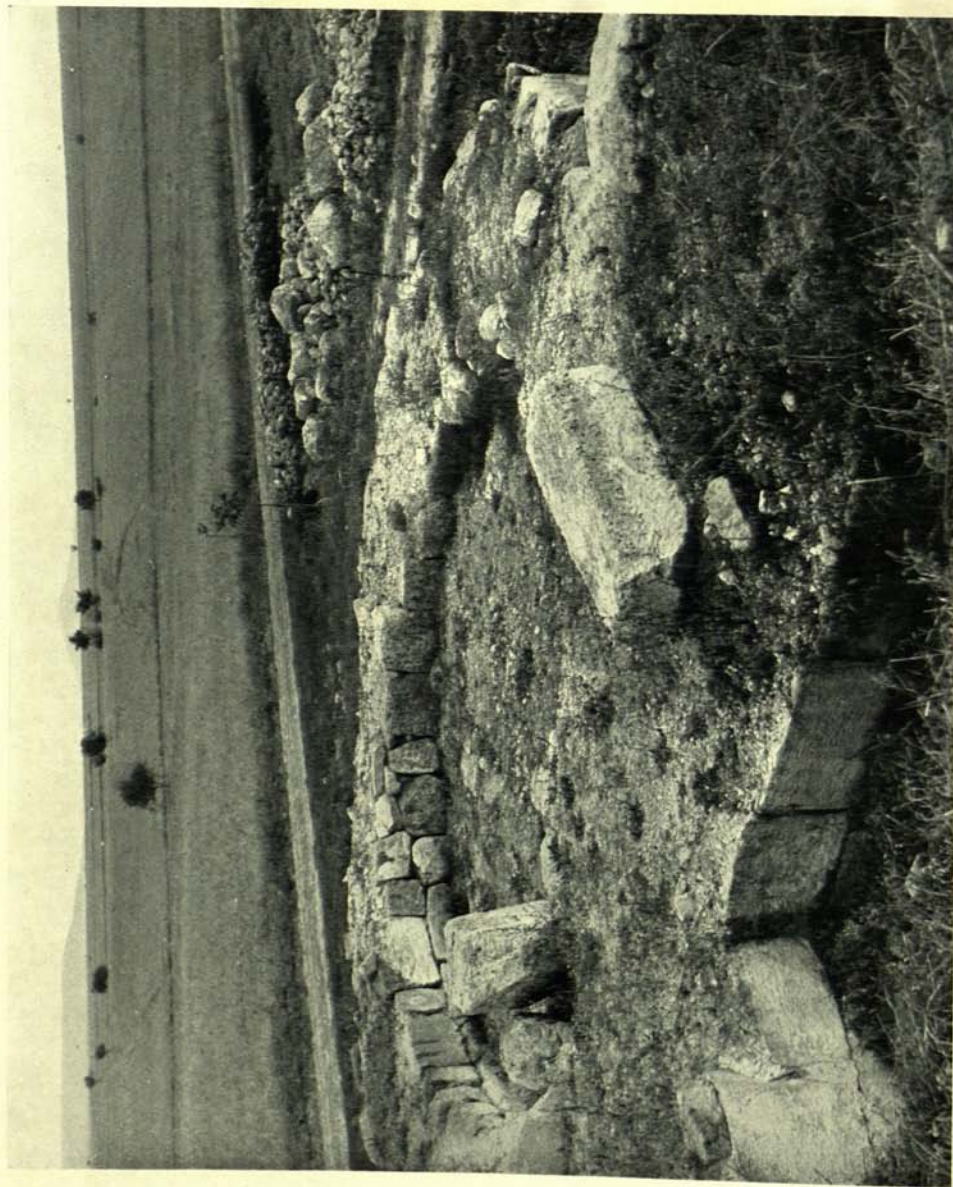
A transverse wall (maximum thickness 1.6 m.) divides the Throne-room into two parts. The northern part is a hall 31.4 m. long,

PLATE I



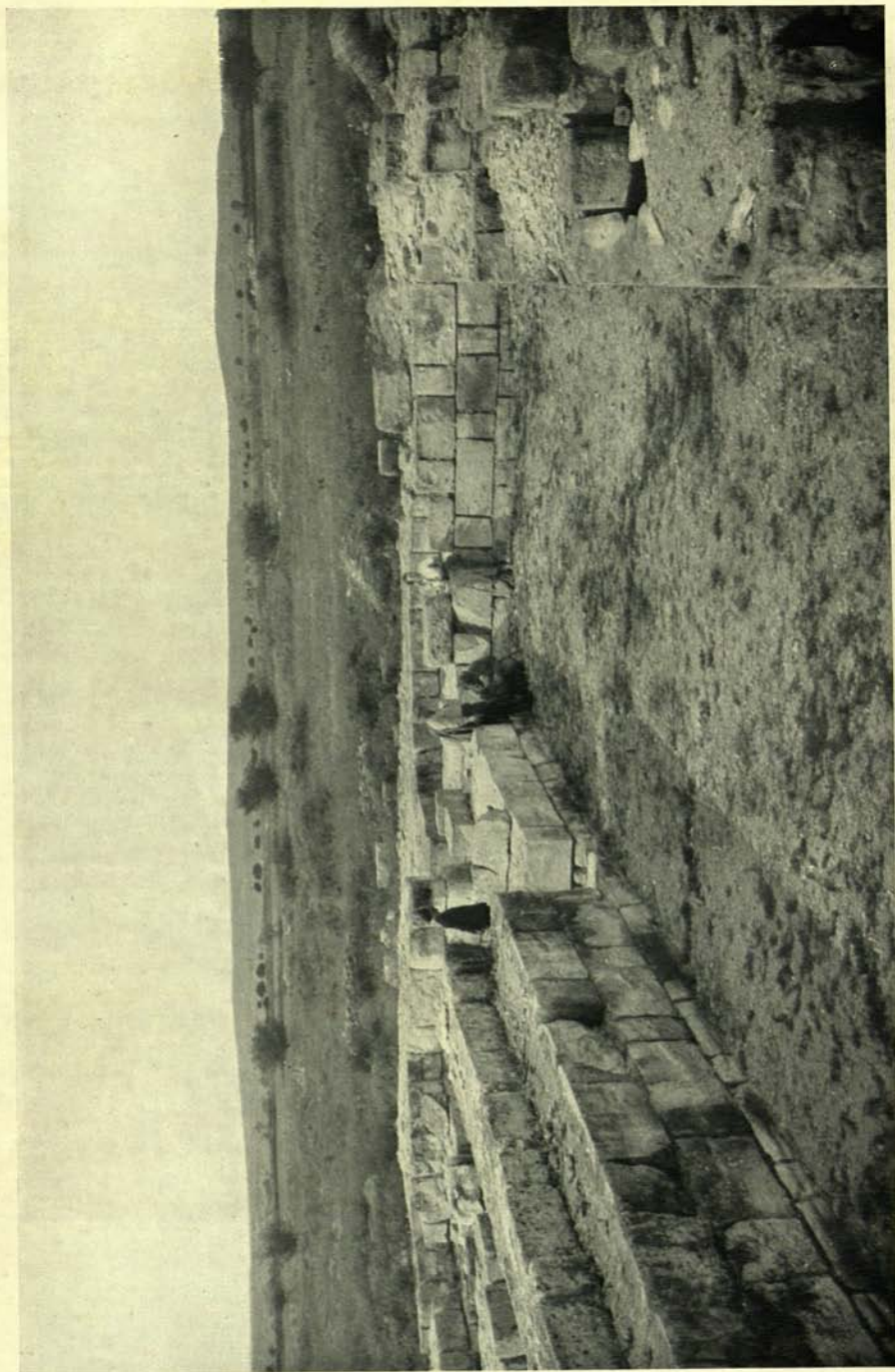
PLISKA : EAST GATE, LOOKING WEST (see p. 296)
Ph. O. G. S. Crawford

PLATE II



PLISKA : ROUND TOWER AT SOUTHEAST CORNER OF INNER FORT (see p. 296)
Ph. O. G. S. Crawford

PLATE III



PLISKA : GENERAL VIEW OF THRONE-ROOM, INNER FORT, LOOKING NORTHWEST (see p. 296)
Ph. O. G. S. Crawford

PLATE IV



INSCRIPTION BUILT INTO WALL OF PALACE AT PLISKA, NEAR SHUMEN, BULGARIA (see p. 301)
Ph. O. G. S. Crawford

PLISKA

consisting of four corridors—flanking corridors are each 3 m. wide, the central west, 4.6 m., the central east, 5.8 m. These corridors were connected by wide entrances, maximum width 2 m., so that it was very

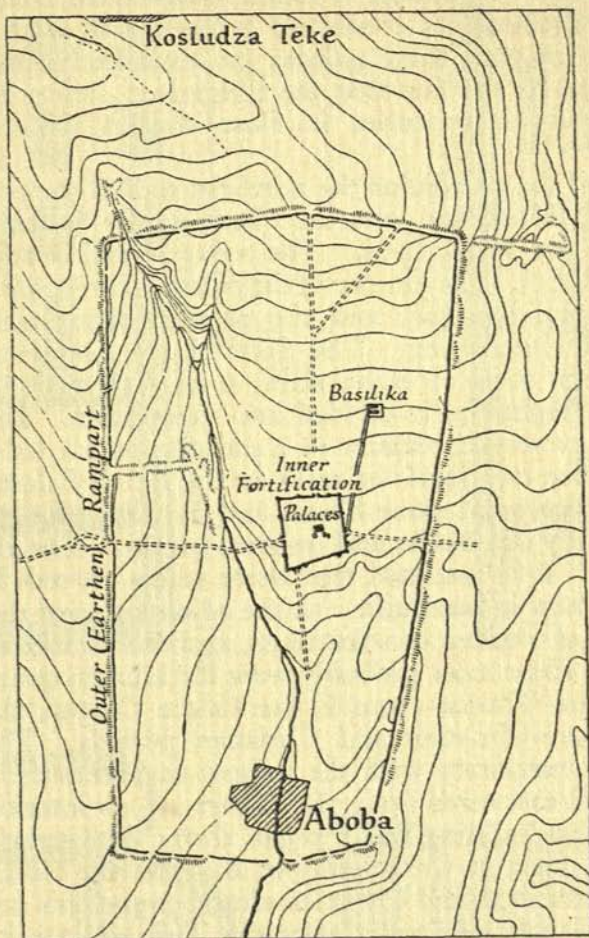


FIG. 2. FORTRESS AT ABOBA (PLISKA), BULGARIA

easy to pass from one end of the throne-room to the other. Most probably the throne stood at the north apsidal end. It is accessible to all the different halls (FIG. 3).

The architecture and solid proportions of the hall have a purely oriental character, very different from that of Byzantine buildings. The

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use of large, square stone blocks was not known in these parts before the coming of the Bulgarians. Traces of similar construction are seen in the oldest parts of the fortifications at Preslav and Tirnovo. The origin of this type of building is found in Sassanian architectural traditions, whose monuments consist of palaces built with similar stone blocks. Even in plan these palaces are completely similar. This is easily explained by the fact that the Bulgarians, before their westward migration, lived for centuries in close contact with the Sassanide Empire.

The Small Palace adjoins the northern wall of the enclosure which surrounds the court and church. Rectangular in construction, its measurements are 23 by 19 m. The western wall is 1.6 m. thick, the others about 0.95 m. The interior of the palace is divided into rectangular divisions and subdivisions, and the narrow corridor-like enclosures are particularly interesting. The floor of the northern enclosure is covered with 12 rows of stone slabs, 6 in each row. The western corridor at its beginning is divided into three parts. In the southern one there are preserved remains of a staircase which led to the second floor. There are two kinds of walls in the Small Palace: the first is built with regularly cut stone blocks and mortar; the second consists both of artificially cut blocks and unhewn blocks. The roof must have been of wood. It is assumed that there was a second floor, but it is difficult to tell how it was built. In the adjoining yard there have been found remains of wooden constructions, and the bones of domestic and wild animals. Here most probably were the kitchen quarters. (FIG. 4).

West of the Throne-room is the Palace Chapel, the remains of which are of pre-Christian and Christian periods. The older construction is contemporary with the palaces and measures 24 by 15 m., with a long wall eastwards (FIG. 5). There are no interior walls. It is assumed that this building was a pagan shrine of the early Bulgarians. After the acceptance of Christianity in Bulgaria the old Pagan temples were usually converted into Christian churches and this process was repeated in the Balkan peninsula after the Turkish invasion, when the Christian churches in their turn were converted into Mosques.

In order to reach the large Christian basilica outside the fortress one must pass along the old paved road, the only one preserved. This road proceeds from the Throne-room, passes through the main eastern gate of the fortress and then turns northwards. It is 2.5 m. wide and is paved with large stone slabs, some of which are 1.8 m. long. The basilica was built in an open space, about 1 km. northeast of the

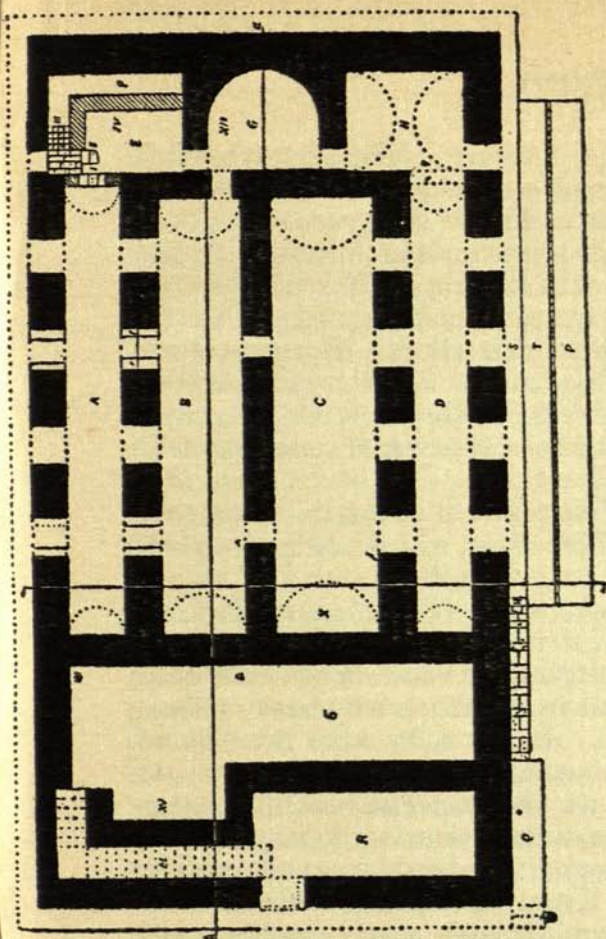


FIG. 3. THE GREAT PALACE, PLISKA

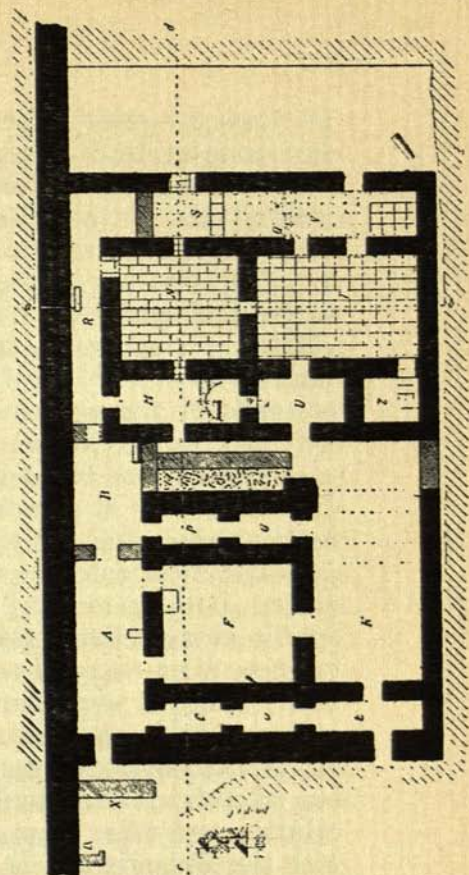


FIG. 4. THE SMALL PALACE, PLISKA

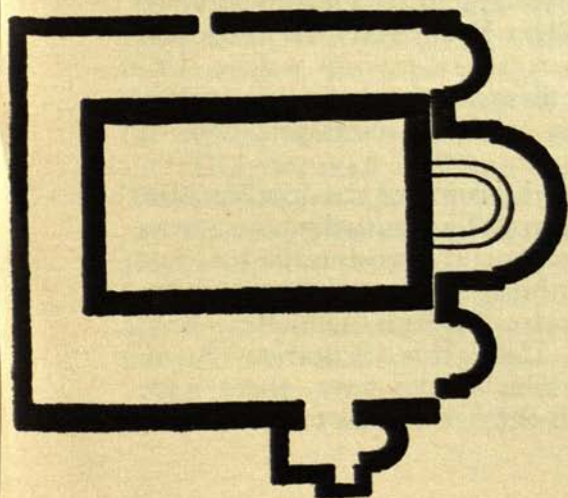


FIG. 5. PALACE CHAPEL, PLISKA

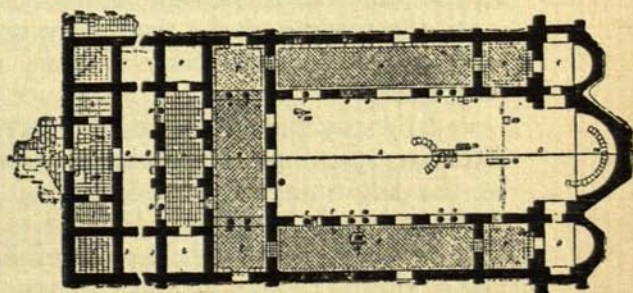


FIG. 6. BASILICA, PLISKA

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fortress but within the great earthen rampart. The site consists of two terraces, the church proper lying on the north terrace, with a southern terrace serving as a court. The church was completely isolated and only the road connected it with the fortress. It was surrounded by a square enclosure not strong enough for defence.

The church, before it was excavated was known locally by the name of Klisse-eri. There is a legend that all the treasures of the church and the royal palace were thrown into a well. According to the same legend this well lay somewhere to the southwest of the southern entrance of the church. A well has been discovered here, but it is northeast of the church.

The basilica of Aboba shows distinct traces of eastern influences. It has the form of a rectangle with three apses, and is 100 m. long and nearly 30 m. wide (FIG. 6).

The main entrance was on the west side. There is a narthex and atrium, both 50 m. long—nearly half the whole length of the building. At the corners there are square constructions which must have been towers. The side aisles are separated from the nave by heavy pillars, between which were marble columns. In the ruins have been found several marble capitals and column bases. They vary in form and ornament, suggesting that they were not made specially for the church but taken from other buildings. The custom of bringing away marble columns and other monuments as booty after a battle was very popular with the Bulgarians. In 814 Khan Krum reached the walls of Constantinople, and on his way there plundered many churches and monasteries. When he returned to Bulgaria he brought away columns and other monuments of Greek art. According to Ireček such columns were placed in front of the palaces of Khan Krum and were used later in the building of the new temples.

In the middle of the nave have been discovered remains of a pulpit, and in the southern aisle a well, most probably connected with a baptistery.

In many places and particularly in the ruins of the basilica have been found columns with short inscriptions. These usually have a cross at the beginning, followed by the name of a town-fortress or, less frequently, the mention of a war. In the basilica of Aboba were found columns with the names of Rodosto, Burdisus, Dimotika, Visa, Arkadiopolis (Liule Burgas), Sosopolis, Mesembria, Scutarion, Seres. Judging from those found in the narthex of the nave, there were originally sixteen such columns. Ireček thinks that they were placed

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in front of the palace to proclaim the number of cities conquered by the Khan; but Uspensky, who directed the excavations, thinks that they were installed in the big church, built by Simeon near the royal palaces about 924, which is also the approximate date of the basilica. Prof. Zlatarski accepts Ireček's conclusion.

The basilica of Pliska is one of the largest of the old buildings found in Bulgaria. We know that during the reign of the first Bulgarian Christian ruler, Boris (852-889), many cathedrals were built. The material of all the public buildings comes from the country around Aboba, Shumen and Devnia. The nearest and perhaps the main source available was the ruins of the Roman city of Marcianopolis. The records of this city are interrupted about the date of the foundation of Pliska.

Among the old Bulgarian inscriptions written in Greek, derived originally from Pliska and its neighbourhood and later removed elsewhere, the most important are the column inscriptions composed in honour of certain men for their services to the government and the people. (PLATE IV). Similar monuments were erected not only after the death of these men, but also during their life-time. On all these monuments, with very few exceptions, occurs the name of Omurtag or Malamir. They are the only documents we possess relating to old Bulgarian social customs, which were extinguished by the introduction of Christianity and the ensuing political changes. These inscribed columns are very important from the point of view of philology and history, and that not only for the Bulgarians, but also for the other eastern peoples of the migration-period in general.

The inscriptions do not vary much in their general content. They generally state the title of the Khan, or other person who ordered them to be written, the name of the person in honour of whom the column was erected, and his position in the government, personal relations with the Khan, merits, origin, and kin.

Thus we read: 'Khan Übigi Omurtag. Ohshuan, the Jupan* Tarkan,* was my adopted man and died in battle. His kin was Kurigir.'

Of particular archaeological importance is the lost inscription of Kavhan Ishbul: 'Khan Jubigi Malamir, prince from God. His old Boila* Ishbul built the aqueduct and gave it as a present to the prince; and the prince gave to the Bulgarians to eat and to drink many times and to the Boils* and the Bagains* gave many presents. May God allow

*These untranslatable words are titles of nobility.

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the prince to live a hundred years together with Kavhan Ishbul'. The Bulgarian Khan Malamir (831-836), son of Omurtag, erected a column in honour of the Velmodza, the Kavhan Ishbul, who built an aqueduct, most probably in Pliska. In the inscription are mentioned the festivals held in honour of the opening of the aqueduct. These were the famous festivals of the proto-Bulgarians held on different occasions, as for instance at the conclusion of an important government undertaking. But the most imposing were those held to commemorate an important personality.

Pliska is the probable source of other important historical inscriptions. Of these we must mention first the column of Suleiman Kōi, which was taken there from Pliska. This monument must belong to the time of Omurtag and contains the important treaty of 814, between Bulgaria and Byzantium. There is a fragment from the basilica with the date 1142 on it (in the reign of John Comnenus); on another fragment is the monogram of the Paleologs, and on another the Slav inscription ДѢЛАНЪ БАНЪ, which is the name of a well known person of the eleventh century. The inscriptions show that these regions did not lose their importance with the coming of Christianity, and that they were not completely forgotten or deserted in the eleventh and twelfth centuries.

In all the buildings are found stones, bricks, tiles, and aqueduct pipes with different signs on them. They are probably not writing, but a magical means to catch and concentrate thoughts on a particular object. From all that is known of the old culture of the proto-Bulgarians it is natural to assume that they had some kind of writing, which consisted of dots and dashes, as mentioned by Černorizetz Khrabr. The problem of the existence of a Slav alphabet before the adoption of the Cyrillic is not new. These mysterious signs still await solution.

The numismatic material from Pliska is not very rich. It consists of a few lead seals and several dozens of Byzantine coins. The most important of the lead seals is the one of Vasili II (976-1025). On the obverse is represented the Emperor. In his right hand he holds the cross and orb, in the other a small sack of earth—the acacia, symbol of the man's transitory existence. Lastly there was found at Pliska a lead seal with a representation on the obverse only and the inscription: Peter, king of the Bulgarians.

In the neighbourhood of Pliska there are several other interesting monuments. Such are the so-called Devtashlari. These are colossal stones standing in regular or irregular groups. In front of these groups is a stone shaped like a throne and facing the group. Excavations

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around the Devtashlari have not yet yielded any definite results, nor have they disclosed the epoch to which they belong. On some of them the proto-Bulgarian sign-writing has been found. They may have served for some ceremonies of which we still know nothing. The stone statues, called Babi, discovered near Endze (in the neighbourhood of Shumen) will bring us closer to the solution of this problem.

It is supposed that Isperrich founded the capital of the Khans. Pliska was first attacked by the enemy in 811. Later we find there the residence of Omurtag, who was the chief builder of the forts and palaces.

It is recorded that in 885 there were in Pliska the three disciples of Methodi: Climent, Naum and Angelari; they were banished from Moravia and sent to King Boris.

Pliska must already have lost its importance as a capital in the time of Boris. This fact is attested after 893 when Simeon was chosen king by a national assembly. These regions must have been invaded by the Magyars in 895, and by Ivan (Zimiscès) in 972, when he took Pliska on his way to Drester.

A second invasion is recorded in 1001 when the leaders of the army of Basil II, after taking Veliki Preslav and Malki Preslav, also took Pliska. Worst of all was the invasion of the Petchenegs in the second half of the eleventh century. Everything was plundered, and the monuments of the Bulgarians were destroyed or carried away. Later the capital of the Bulgarian state was moved elsewhere. Pliska and Preslav, plundered and destroyed, lost their political and cultural importance for ever.

Ras Shamra, 1929-39*

by T. H. GASTER

IT is now ten years since the French archaeological expedition under M. Claude Schaeffer first started digging at Ras Shamra, on the north coast of Syria. The present is therefore a suitable moment in which to take stock of what has so far been discovered.

Our best point of departure is provided by the series of annual volumes in which the excavator's reports and the epigraphists' primary studies have been conveniently gathered together from the pages of the periodical SYRIA. It must be borne in mind, however, that these excellent publications represent a *preliminary* and not a *final* stage in the interpretation of the materials. The literature grows from day to day, and the contributions made by other scholars are equally important for a correct appreciation of the subject. Among these special mention should be made of the work of Albright, Montgomery, Harris, Goetze and Gordon in America, of Ginsberg and de Vaux in Palestine, of Friedrich and Eissfeldt in Germany, of Hrozný in Prague, Aistleitner in Budapest and Cassuto in Italy. In this country studies have been published by the late Father Burrows, Dr J. W. Jack, of Edinburgh, Professor S. H. Hooke and the present writer.

STRATIFICATION OF THE SITE AND HISTORY OF UGARIT

It is now established, on the basis of texts found there, that the mound of Ras Shamra conceals the ancient city of Ugarit, long known to students both from the Amarna Letters and from sundry references in Egyptian and Hittite records.

Ugarit possessed one of the few natural harbours on the Syrian coast, and it is but a day's sail from the important island of Cyprus, the

* Claude F. A. Schaeffer and Ch. Vroilleaud. *Les Fouilles de Minet el Beida et de Ras Shamra (première campagne)*, 1929. The second to the ninth reports were published in SYRIA, 1931-8 (Geuthner, 12 rue Vavin, Paris).

Claude F. A. Schaeffer. *The Cuneiform texts of Ras Shamra (Ugarit)*. The Schweich lectures of the British Academy, 1936. Oxford University Press, 1939. pp. xvi, 100, 39 plates. 8/6.

jutting promontory of which lies almost directly opposite. It was therefore destined to serve as a trading-centre between East and West. Merchants from all parts poured into it, and it served as the terminus for caravan-routes leading variously to Palestine in the south, Mesopotamia and the Hurro-Mitannian territories in the east, and Asia Minor in the north. It is this constant crossing and blending of cultures which created the peculiarly syncretistic civilization of the city during its heyday, and it is this which gives it today its primary interest. Ugarit must be regarded as a kind of Cairo of the ancient world.

Archaeological evidence points to occupation of the city from neolithic times down to the twelfth century B.C., but the exact course of its history cannot yet be determined. Details of the stratification, tabulated from M. Schaeffer's excellent reports, are given on the accompanying table. It must be observed, however, that the latter does not profess to set out every detail, but merely to present the more essential features of each level.

From the table (see page 306) it will be seen at once that the heyday of the city, and the period concerning which information is most abundant, falls during the epochs of the Fourth and Fifth Settlements, extending approximately from the twenty-first to the twelfth centuries B.C. and corresponding to the age of the Twelfth and Eighteenth Dynasties of Egypt.

The preceding periods (First-Third Settlement) are extremely obscure. Certain cultural contacts are perhaps suggested by the affinities of the pottery with those found at other North Syrian sites, but such evidence must necessarily be treated with reserve. We know far too little about the origin and distribution of the types concerned to justify the conclusion that their occurrence at any two sites proves an immediate relationship between them. There were probably several intermediate stations and future excavation can alone settle the question.

An interesting feature of Second Settlement pottery is the tendency to use in decoration the *motif* of a row of human eyes. This recalls the employment of a similar motif in the early architecture of Brak and, if it be really apotropaic in character, as Schaeffer suggests, it provides important evidence for the antiquity in Syria of the belief in the Evil Eye.

From the period of the Second Settlement onwards, the influence of Egypt is especially dominant, due, no doubt, to the results of the policy of imperialist expansion pursued by the Pharaohs at this period. The copper mines of the Lebanon area, and the plentiful supply of cedar-wood available in the district would naturally encourage such ambitions,

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RAS SHAMRA—UGARIT—STRATIFICATION

Level	Particulars	Date
I	(a) Cosmopolitan culture ; invasion of ' sea-peoples ', (cf. Malalas' story that Kasos, <i>i.e.</i> eponym of Casiotis region, married Kitia, eponym of Kition).	XIIth cent. B.C.
	(b) From XIVth cent. Mycenaean ware is prominent. Rectangular tombs with vaulted ceiling and small <i>dromos</i> . Hurrian influence abounds.	XIV—XIIIth cent. B.C.
	C o n f l a g r a t i o n	
	(c) From XVth cent. onwards, tombs with exclusively Cypriot ware. <i>Fifth Settlement</i>	XV—XIVth cent. B.C.
II	Distinguished by profusion of ' Phoenician ' ware. Two Semitic temples, one dedicated to Ba'al and the other to Dagan. Bronze ware from Cyprus. Level dated absolutely by coralline pearl bearing name of Sesostri I, and by statue of the wife of Sesostri II (1906—1887 B.C.). <i>Fourth Settlement</i>	XXI—XVIIIth cent. B.C.
III	(a) Painted ware—dark brown, violet or black on buff background—thin—unparalleled on Syrian coast, but possesses counterparts at Tepe Giyan and Susa I <i>bis</i> , as also in ware found by Ingholt at Hama. Designs sometimes incised on clay whilst still wet. Polished axes. Obsidian blades.	Third Millennium
	(b) Unpainted pottery with red glaze. Also, distinctive polychrome pottery, red or black on buff clay. Jar-burial. <i>Third Settlement</i>	First half of Fourth Millennium B.C.
IV	Painted pottery : triangles—zigzags—vandykes—meanders—dots—frets—rows of human eyes. Vessels are globular with flat bases—horizontal handles—black lustre on dull clay—incised and scratched designs. Metal unknown. Blades of silex. Small axes in hard polished stone. Bone bodkins and awls. In earlier level : thick dull clay ware, with yellow or bright red glaze. Reminiscent of so-called ' neolithic ' ware from Cyprus (ANTIQUITY, 1934, VIII, p. 86). Earlier period has parallels at Sakje Geüzi, Khan Yunus, Samarra, Susa I and Tepe Mussian. <i>Second Settlement</i>	First half of Fourth Millennium B.C.
V	Unpainted ware—ledge handles—implements in flint (silex) and obsidian. Primitive ornamentation of vessels by means of scratched vertical lines, irregularly parallel, or simple incised patterns. <i>First Settlement</i>	Neolithic

whilst the control of so important a harbour would be of distinct commercial advantage.

Positive evidence of this influence during the period of the XIIIth Dynasty (Fourth Settlement) is afforded by the discovery, south of the great temple, of a broken statuette of Chnumit, bride of Senusret III (1906-1887 B.C.), whose tomb has been unearthed at Dashur. To the same period belong two sphinxes inscribed with the name of Amonemhet III, and a group featuring three persons, one of whom is the XIIIth Dynasty official Senusret-onkh.

That Ugarit was also in contact at this period with other important states is proved by a letter, recently discovered at Mari (Tell Hariri) and dating about 2000 B.C., in which a certain Hammurapi, king of Kurda (*not* the great monarch of Babylon) informs Zimrilim, king of Mari, of a desire expressed by the governor of Ugarit to view the beauties of the latter's palace (Dossin, SYRIA, XIX, 125 f).

In the time of the XVIIIth Dynasty, our information concerning the history of Ugarit is eked out also by external records, most important of which are the celebrated Amarna Letters. One of these, addressed by Abimilki of Tyre to Akhenaten informs him that half of the city of Ugarit has been consumed by fire (Winckler, 151, 55). M. Schaeffer has ingeniously suggested that this fire may be identical with the conflagration the traces of which separate the second from the third layer in Level I. He has furthermore pointed out that in the ruins of a house dating just before the conflagration was found a scarab inscribed with the name of Amenhotep III who died in 1376 B.C.

Later, in the XIXth Dynasty, the name of Ugarit turns up again among the allies of Rameses II (1301-1234 B.C.) in his battle against the Hittites at Kadesh (1283 B.C.). After that, it disappears from the pages of Oriental history, except for sporadic and unimportant references in the Hittite documents from Boghaz Kewi. The prevailing view is that it fell a victim to the Syrian campaigns of Tiglath-Pileser I (c. 1100 B.C.). Before this occurred, however, the Hurro-Mitannians had already gained supremacy. The only two kings whose names we know, viz. Niqmedaš and Astari, bear Hurrian names, and in the ritual texts discovered on the site (v. *infra*) Hurrian deities are mentioned side by side with those of the Semitic pantheon. Moreover, at least 40 per cent of the proper-names occurring in those documents are Hurrian and, as will be described presently, texts written in a language closely resembling, if not identical with, Hurro-Mitannian turn up among the written archives of the city.

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Ceramic evidence shows that early in the period of the Fifth Settlement the influence of Cyprus began to make itself prominent. Similarities with the so-called 'neolithic' ware of this island occur already in the time of the Second Settlement (ivth Level from the top), but it is only at this late period that the influence becomes really marked, and it is then coupled also with evidence of Mycenaean contacts. It is probable, therefore, that Ugarit, like many another coastal city, felt the pressure of the Sea-peoples, but the exact extent of their domination cannot yet be determined. The present writer is inclined to suggest, however, that ethnic conditions such as must have obtained at Ugarit during the later centuries of the First Settlement are portrayed by the North Syrian seer Balaam in the celebrated passage, Numbers XXIV, 24. This passage, the antiquity of which is, despite its later redaction, surely to be recognized, runs as follows in the Revised Version :

But ships shall come from the coast of Kittim (Kition, in Cyprus), and they shall afflict Asshur (Assyria) and shall afflict Eber (the Hebrews).

It is, however, quite absurd to speak of Cypriot ships as oppressing far-distant Assyria. Therefore, the writer would suggest that Asshur and Eber are *subjects*, not *objects*, of the verb 'oppress'. The sense will then be :

There shall come ships from Kition, and both Assyria and the Hābiru shall bring about oppression.

This corresponds exactly to the situation in North Syria during the period in question. On the one hand was the invasion of the Sea-peoples and of the marauding Hurrian and Semitic Hebrews, and on the other the later conquest by the Assyrian forces of Tiglath-Pileser I. If the passage were written at any time after 1100 B.C., it would reflect most faithfully the troubled state of North Syria at this era of its history, and might thus be taken to portray the conditions in which Ugarit then found itself.

This is as far as we can reasonably go today in sketching the history of Ugarit.

THE TEXTS

More important than anything else discovered at Ras Shamra are the Cuneiform Tablets. These all come from the First Level, and from a layer below the remains of the great conflagration. They are therefore in any case older than 1375 B.C. Some of them, containing mythological poems, were found in the precincts of the temple-library

and may be dated approximately about 1500 B.C., though their actual contents, being traditional, are no doubt older still. Others, unearthed at different parts of the site, are not of uniform date. Some may be earlier than the sixteenth century, some later. It is impossible to tell. All, however, belong to the same general epoch of the Fifth Settlement.

The texts divide readily into two main classes, viz. (a) those written in the usual *syllabic* cuneiform, and (b) those written in an hitherto unknown cuneiform *alphabet*. The former are written both in Accadian and in a language, as yet imperfectly understood, akin to Hurro-Mitannian. The language of the latter may best be described as proto-Hebrew. It is an earlier stage of the language in which the Old Testament is composed, and consists of a blending of several Semitic dialects, some North-Western, or Amoritic, and some Southern, or Arabic.

The origins of the cuneiform alphabet are as yet unknown. There are two principal theories. The one, propounded by the German Assyriologist Ebeling, asserts that the signs are in reality nothing but curtailments of normal syllabic characters. By omitting a wedge here and a wedge there signs which, say, originally stood for *ma* or *pa* were made to do duty for the corresponding alphabetic values *m* and *p*. The other theory, sponsored by Olmstead, the late Father Burrows and the present writer, asserts that the Ras Shamra script is an adaptation to the exigencies of wedge-writing of that ancient Semitic alphabet, parent alike of the later Phoenician and Greek systems, traces of which have been recovered in recent years both at Serabit al Khadim in Sinai and at various Late Bronze Age sites in Palestine, such as Tell Duweir, Beth Shemesh, Gezer and Shechem. It must be confessed, however, that as yet neither school of thought has been able to produce a connected epigraphic chain in support of its thesis, and the problem becomes increasingly complicated by the succession of new scripts which are constantly turning up in Syro-Palestinian sites, as for instance at Byblus.

One objection which has been raised against the second theory, and which in some quarters has been regarded as decisive, is the fact that the Ras Shamra script possesses three distinct signs for the letter *aleph*, depending respectively on whether it is vocalized as *a*, *i* or *u*, whereas the palaeo-Hebraic alphabet has but one sign. This objection is, however, by no means final, for the fact is that two of the Ras Shamra *aleph*-signs are but secondary formations from the letter *h*, and therefore of later introduction. In the original cuneiform alphabet, therefore, there was only one *aleph*-sign, as in the palaeo-Hebraic, and it is to

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the original form of the script, not to its development at Ras Shamra, that we must turn when we seek to determine the question of origin.

Be this as it may, and whatever the ultimate origin of the script may have been, it is noteworthy that another specimen of it, though written retrograde, has been found in the South Palestinian site of Beth Shemesh. This at least shows that it was not exclusive to Ugarit. Moreover, the very fact that it is there written retrograde, as also on a few Ras Shamra documents, might point to an origin in some system of writing, like that of Serabit al Khadim, wherein the letters could, in fact, face in either direction.

The contents of the Semitic tablets are extremely varied. Several of them, discovered during the first campaign of 1929, are ritual tariffs, defining sacrifices to be offered in the temple. They are especially interesting because many of the sacrifices bear names long familiar from the codes of the Pentateuch showing that, however late be the literary redaction of that work, the material incorporated is genuinely ancient and belongs to that early Hebrew cultus from which the Ugaritic and the Israelitic (as well as the Phoenician and later South Arabian) systems were alike evolved.

Another interesting feature of these texts is that in some cases a line is drawn across the tablet after the main body of their contents, and thereafter is specified expressly the part to be played in the ritual by the king. This shows that, as in Mesopotamian cultus, the king played a distinctive role in temple-ceremonies, and it further recalls the later Israelitic distinction between the sacrifices offered by the king and those offered by the people (cf. II Kings, XVI, 15). In one document (IX, 10-11) we actually hear of the king's performing a ritual act of purification (*m-l-k b-r-r*) in the new-year month of Teshrit (*y-r-h T-š-r-t*). This perhaps suggests a ritual period of purification preparatory to an annual renewal of kingship at the season of the New Year—a custom which is not without parallels.

Another text (no. III) appears to prescribe sacrifices for a seven-day festival, and this precedes the day of new moon (*y-m h-d-š*). It is therefore probable that the ritual in question was designed for the great week of celebration which marked the beginning of the year, for according to the recent conclusions of Professor Julian Morgenstern the ancient Hebrews observed this occasion just before the new moon.

We also read in these ritual texts of offerings on the roof-tops, such as were denounced by the Israelitic prophets, and likewise of the erection to the deities of cathedrae, or little seats (*m-š-b-t*), recalling the 'seat'

(*mōšab*) of the Idol of Jealousy seen by the prophet Ezekiel in the fore-court of the Temple at Jerusalem, and denounced by him as a heathen practice (Ezekiel, VIII, 3).

A particularly interesting document (no. XVII) gives a list of deities worshipped at Ugarit, and in this list we find such Semitic gods and goddesses as Astarte, Resheph, 'the god of the North' (*E-l Š-p-n*) and Šalem (whose name survives in Jeru-salem) honoured side by side with members of the Hurrian pantheon like Ishkhara and the obscure D-d-m-s, reflecting the mixed population of the city at this period.

Besides the ritual texts, there are also other minor documents of great interest. One of these is a treatise on the cure of equine disorders, among which is mentioned the bronchial condition still known as 'roaring'. Herbs prescribed in remedies include sesame, juniper, licorice, and mint. A poultice of figs is also mentioned.

Another tablet contains a schoolboy's exercise in grammar or spelling, consisting of a list of words (mostly verbal forms) beginning with the letter *y*. Such texts have also been found at Babylon and published by Pinches.

Further, there is a deed for the transference of a house left as a legacy (no. xv). A certain Manimu and his son append their 'hand' in witness.

Important also are certain long lists of personal names followed by those of the cities from which these persons came. The lists were probably prepared for purposes of land-taxation, and throw interesting light both on the onomatology of the Ugaritians and upon their political and commercial contacts. The names are partly Semitic and partly Hurrian. The former are distinguished by their resemblances to the nomenclature of the Western Semites, or Amorites, and the South Arabians, especially noticeable in the tendency to add the termination *-ān(u)* to hypocoristica, as in the names of the Hammurabi Dynasty. The latter are often identical with names found in the documents unearthed at Nuzu. The toponyms include Byblus (*G-b-l*, the *Gebal* of the Old Testament), Wassugani, capital of Mitanni (*U-š-k-n*) and Hobah, near Damascus, mentioned in Genesis XIV, 15.

One of the smaller texts (no. 11) contains what appears to be the ritual for the conclusion of a political alliance between the people of Ugarit and certain surrounding nations, among whom mention is made of the Hittites, Hurrians, and Subareans, as well as of the inhabitants of Alše (the classical Arazene, or perhaps Alashia=Cyprus?) and of Dadmush (?). They are described as peoples who 'agress, oppress and

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distress'. The text presents the same virtually, addressed separately (to judge from the verbal and nominal forms) to women and to men. It is distinguished by the fact that every salient phrase is repeated thrice in synonymous terms and that each paragraph concludes with a reference to offerings presented to the deities. The text therefore represents, in all likelihood, the text of a protocol to which the men and women of the city were solemnly sworn. What makes this conjecture the more plausible is that each paragraph ends with two mysterious words which may be rendered 'here is the wild ass', and from documents discovered at Mari we learn that the expression 'to slay a wild ass' (the same word is used in the original) was a regular expression for 'to conclude a political agreement'.

Interesting also is a short letter addressed by a certain Iwri-zar to his friend P-l-s-ya (vocalization uncertain) enquiring after the true circumstances of two men who have complained that 'the hand of God', being 'vehement like death', has touched them adversely.

There is also an interesting list of temple-officials, from which we learn that the sanctuary maintained a regular staff of masons, smelters and smiths.

Before passing on to describe the long mythological poems, a few words may be said concerning the documents written in Accadian and in syllabic script.

One of these is a commercial document dealing with transactions in *argaman*, or purple-dyed material. This may have been a regular article of commerce between the people of Ugarit and those of Cyprus, which lies almost opposite, for it is noteworthy that even in the days of the prophet Ezekiel (xxvii, 7) *argaman* was regarded as the special product of 'the coasts of Alashia-Cyprus'.

Other documents written in Accadian deal with transfers of property and with the release of a slave respectively. From the latter we learn the interesting detail that it was part of the ceremony of manumission to pour oil upon the slave's head (*tabak šamna ana qaqqadiša* : RS. 8208, 7-8).

There are also fragments of a well-known and standard Sumerian-Accadian syllabary.

Of the texts written alphabetically in non-Semitic speech it is difficult to speak, since the language in which they are composed is as yet imperfectly understood. One of these documents contains a list of deities whom Hrozný has recognized as members of the Hurrian pantheon, mentioned again in various texts from Boghaz-Kewi. This fact, taken

together with certain linguistic phenomena, has established the conclusion that the language is akin to Hurro-Mitannian. Several words found in Hurrian texts (such as *arte* 'city', *awari* 'field', *turuḫ-* 'male', *ašte* 'woman, female', etc.) have already been recognized, and it has been found possible, on the basis of the alphabetic spelling, to clarify many points of Hurrian phonology somewhat obscured in the rough and ready syllabic script in which all Hurrian texts hitherto known are written. In the elucidation of these texts the work of Speiser (on phonology), Friedrich (on representation of sounds) and Brandenstein (on vocabulary) has been especially noteworthy. The present writer has also contributed to the subject by an analysis of the inflexional forms to be found in the text and by various combinative suggestions designed to elucidate the meaning. It is his intention shortly to publish a fuller study, correcting previous errors of his method and attempting to carry the investigation to a more decisive issue. At the present moment, however, little can be said beyond the fact that the language, though undoubtedly of the Hurrian family, is not absolutely identical with the Hurrian spoken, or written, at Boghaz-Kewi. It possesses its own dialectical peculiarities.

Concerning the nature of the language, it is apparent that the personal subjective suffix is *-š*, that the impersonal ('neuter') plural, both nom. and acc. ends in *-n*, that the verbal noun is formed by the suffix *-r*, and that a verbal suffix is *-žmk*. The enclitic *-m* marks the copula, and *-h* is the termination of demotica. Beyond this, it is scarcely possible to go.

One of the texts is a long litany, in which address is made (always in almost identical terms) to a number of deities, grouped together in a single all-embracing formula at the end. Most of these deities are known members of the Hurrian pantheon, such as Teshub of Hallab (Aleppo), Allanis, Iršappi, Imhu-Nikkal, Nupatik, Pisasaphi and Simige. Mention is also made in this text of a certain '*-n-t a-m-r-n* who is probably the goddess 'Anat, the Amorite'.

Two other of the non-Semitic texts are almost identical with one another in phraseology, except that each contains different proper-names. They probably commemorate offerings. Another contains a long list of peoples, repeated several times, and interspersed with words which appear to be Semitic. The present writer believes that this is a protocol like the Semitic treaty of alliance mentioned above.

Considerable help in the elucidation of this language may be found in a judicious use of a trilingual glossary found at Ras Shamra, the three

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columns of which are respectively, Sumerian, Accadian and an agglutinative non-Semitic tongue resembling Hurrian.

An attempt to elucidate the new tongue has recently been made by Bork, one of the pioneers in the interpretation of Mitannian.¹ In the opinion of the writer, however, this attempt proceeds from an entirely unfounded basis and pursues a course more distinguished by ingenuity than by sobriety. This is also the opinion of other more competent authorities.

Turning now to the long mythological poems, we shall first describe their contents and then say a few words about their general character and import.

First and foremost comes the cycle known as 'The Lay of Baal'. The theme of this cycle is the rivalry which exists between the two gods Aleyan-Baal (*i.e.* 'Baal the Puissant') and Mot (*i.e.* 'Death'). The rivalry involves a continuous struggle for dominion over the earth, each alternately defeating the other. The several tablets which belong to this cycle describe the various phases of this struggle.

On the one hand, there are passages which describe how Aleyan-Baal, with the help of his sister and consort, the war-goddess 'Anat, succeeds in vanquishing Mot and banishing him from the earth. On the other hand, there are passages (SYRIA, XVI, 247-266) which describe how the supreme god El, whose favourite Mot is said to be, lures Baal to his doom whilst out hunting, and keeps him languishing in a marsh whilst all fertility and vegetation vanishes from the earth.

The myth really symbolizes the alternation of the wet and dry seasons in the Syrian year. Baal represents Rainfall and Fecundity, whilst Mot represents Drought and Sterility. Indeed, the nouns *ba'al* and *mot* are used respectively in these two senses in Arabic and later Hebrew usage.

In all likelihood, these texts were not purely mythic in character, but were the *libretti* of sacred pantomimes acted, as in many parts of the world, at the seasonal festivals. On this hypothesis, the several incidents of the myth would be projections from ritual actions, and by probing beneath the ostensibly mythic surface, it will be possible to reconstruct in large measure the ritual of these proto-Hebrew festivals.

The results of such an enquiry are surprising. We find that the Ras Shamra festivals exhibit that same 'ritual pattern' which has been

¹ DAS UKIRUTISCHE: Die Unbekannte Sprache von Ras Schamra. Die Grundlagen der Entzifferung. Von Ferdinand Bork. (Mitteilungen der Altorientalischen Gesellschaft, XII, 1, 1938).

detected also in Babylonian and Israelitic cultus, and which has been postulated, by Professor S. H. Hooke and others, as the common basis of seasonal ceremonies throughout the ancient Near East. The essential elements of this pattern are: (a) a battle-royal between two rival powers, each claiming dominion over the earth; (b) the formal installation of the victor as king; (c) the erection of a new habitation for him; (d) the celebration of a seven-day festival.

We need not accept all of the conclusions of the 'ritual patternists', nor endorse the superstructure of fancy which they have reared upon their data, in order to see that the essential basis of their theory is indeed confirmed by the Ras Shamra texts.

Other tablets, not themselves part of the Lay of Baal, but doubtless associated with the same ritual ceremonies, fill in the picture.

One of these (SYRIA, 1935, 29-45) describes a battle between Baal and his confederates against the angry forces of the sea and the river. The latter are defeated.

The point of this myth is clear enough. Baal is the god of the rain. Sea and river are therefore his rivals since, by affording an alternative source of moisture to the parched soil, they constitute a challenge to his absolute sovereignty over the earth during the autumn months. Accordingly, before Baal can assert that sovereignty, he is obliged to 'put them in their place'. This is what our text describes. The Mesopotamian myth of the great god's victory over Apsu is a direct counterpart. Transferred to Yahweh (Jehovah), the myth reappears in the ninety-third psalm and in the final chapter of Habakkuk.

Another tablet (SYRIA, 1933, 128-151), this time definitely interspersed with rubrics, contains a series of mythological extracts recited as an accompaniment to the performance of various ritual acts. In this text, we hear first of a rite wherein sacred women went down to the sea-shore, filled basins with water and then returned to deposit them (or pour them out?) in the temple. This is a rain-charm pure and simple. Lucian, in his treatise 'On the Syrian Goddess', described it as taking place twice yearly at Bambyke, whilst the Jewish Mishnah records it as a regular part of the ceremonies at the Festival of Ingathering in Jerusalem. Next, we hear of a rite in which a kid is to be seethed in milk, and in the Pentateuch we read of the same rite as expressly forbidden to the Israelites, in connexion with the ceremonies of presenting first-fruits to the deity (Exodus, xxiii, 19; Deut., xiv, 21). Then is mentioned the erection of certain *cathedrae* (*m-š-b-t*) for the gods, followed by the recital of a hymn beginning with the words '*I am*

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jealous for the gods'. This links up with the vision seen by Ezekiel, just before the New Year festival, in the courtyard at Jerusalem. There was erected, we are told, at the gate of the temple, a *cathedra* (*m-š-b*) dedicated to the 'Idol of Jealousy' (Ezekiel, VIII, 3). This is followed by a lament, on the part of the sacred women, for the discomfited and emasculated Mot, and here again we find an echo in Ezekiel's statement (VIII, 14) that part of the heathen ritual which he beheld consisted in a lamentation for Tammuz (*i.e.* the slain god, here equivalent to Mot) on the part of certain sacred women. Lastly, we read in this text of a sacred marriage, such as recurs in corresponding Mesopotamian rituals, followed by the birth of seven sons, the greatest of whom, destined apparently to restore the fortunes of men, is Šaḥar, the god of the Dayspring (Hebrew *šahar* 'dawn'). Here, once more, we find a parallel in the heathen ritual described by Ezekiel, for we are there told that part of the ceremonies consisted in a ritual act wherein men turned to the East to greet the rising sun.

Thus, it is apparent that our Ras Shamra text affords the prototype of New Year rituals still surviving in Jerusalem in the 6th century B.C. The parallelism strongly supports the contention that the Ras Shamra documents are the *libretti* of sacred pantomimes, behind which lies the picture of proto-Hebrew seasonal ritual.

Belonging to the same cycle are a series of extracts, recently published under the convenient title of *La déesse 'Anat* (Geuthner, 1938) which describes the exploits of the war-goddess in avenging the discomfiture of Baal, and her successful efforts to bring him back from exile and install him as king of the earth. Several points of similarity with the Syrian Adonis-cult, hitherto known only from late Classical sources, herein make their appearance, and it would seem that, as in other parts of the world, the combat against the coterie of Mot is here historicized into the commemoration of an ancient battle between the people of Ugarit and certain hostile neighbours living in the Orontes Valley.

Of quite a different order is the so-called 'Legend of Keret'.² This legend describes a battle between the forces of an ancestral hero called Keret and a certain unnamed adversary. The text has received especial attention in view of a theory advanced by its first editor Virolleaud to the effect that the adversary was named Terach, and that the names of his wife and mistress were respectively Sin and Nikkal, two deities associated with the city of Haran, from which the Biblical Terach,

² *La Légende de Kéret* (Geuthner, Paris).

father of Abraham, is said to have migrated into Canaan. Virolleaud therefore thinks that our text may contain an early Canaanite account of the Terachid invasion of Palestine. This theory, sponsored also by Dussaud and others, is, however, now seen to rest on a false interpretation of certain words in the text. The present writer, who formerly supported this theory at some length, renounces his previous position. *Dies diem docet*. The word *terah* does not mean 'Terach' in the crucial passage, but is a common noun denoting 'bride-price' (Accadian *terhatu*), whilst the words rendered SIN and NIKKAL have quite a different sense, let alone the fact that in Mesopotamia the former is a god, not a goddess! Similarly, Virolleaud's detection in this poem of the names of ASHER and ZEBULUN also rests upon a false exegesis. Thus, the Biblical connexions of this text disappear entirely under the searchlight of a more exacting criticism, and the oft-repeated statement that Terach is mentioned in the Ras Shamra texts is shown to be inaccurate. The merit of this demonstration rests in the first place with Albright, though this does not mean that his own explanation of the passages in question need necessarily command assent. The historical basis of the Keret-text cannot yet be determined, and though most of the philological difficulties can now be removed, it is still impossible (especially since portions are still to be published) to seize the connected sense.

Another important text is that entitled 'The Legend of Danel'.³ The story is as follows:—

An ancient hero Danel has no son. He therefore prays to the supreme god El to grant him one, promising that the child will serve the deity in his temple (cf. the vow of Hannah regarding Samuel). Through the intercession of the god Baal, this request is granted, and Danel begets a son called Aqhat.

The autumn harvest, at the end of the dry summer season, is now drawing nigh, and Aqhat is destined to be lord of the summer crops and to receive tribute of firstfruits in respect of his regal status. This excites the jealousy of the goddess 'Anat who feels that the privilege belongs by rights to her own heir, not yet born. She therefore decides to attack Aqhat, but upon his presenting to her rich presents and undertaking to relinquish his claim, she is persuaded to desist from this course. Nevertheless, for some obscure reason, 'Anat's henchman Yatpan renews the attack and kills Aqhat.

³ *La Légende phénicienne de Danel* (Geuthner, Paris).

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The desolate father appeals to El who vouchsafes an omen proclaiming the future resurrection of Aqhat. This is later accomplished, after various rites of mourning have first been instituted. The whole earth then brings tribute.

The text is in many places extremely obscure, and one cannot lay one's hand upon one's heart and swear that the above summary is correct in every detail. It must be taken, at most, as giving *one view* of the general sense, and the writer is quite prepared to modify it, or abandon it altogether, when more light is available. Only by being quite candid about the provisional nature of our results can any progress be made in this very difficult field of study.

Lastly, there is a short text which appears to be a prothalamium sung at the marriage of a girl named P-r-b-ḥ-š. Into this poem, by way of graceful compliment, is introduced a mythological passage describing how the moon-god Yarikh once wooed and won the goddess Nikkal. The text is especially interesting because it describes in some detail the ceremonies of marriage among the inhabitants of Ugarit.

Such then, apart from a few minor fragments which are too mutilated for general consideration, are the celebrated Ras Shamra Texts. It remains only to add that their language contains innumerable points of similarity with the idiom of the Old Testament, and that at times they recover for us the fuller forms of myths (like those of Leviathan) to which only passing allusion is made in the literature of Israel.

From the point of view of Biblical study, these documents possess extraordinary importance as recovering to us for the first time contemporary evidence concerning the culture of that world in which the constituent tribes of Israel were nurtured. They open up the far-reaching question: How much of the old Testament is really Israelitic and how much of it is merely the survival of proto-Hebrew culture seen through the prism of Israel's distinctive experience? This, it is safe to prophesy, will yet prove the dominant issue in Biblical studies during the next ten years. But first of all, the Ras Shamra texts will have to be more fully elucidated.

The documents are also important from the point of view of Classical studies. The present writer has elsewhere pointed out that the long mythological texts, based as they are upon seasonal pantomimes, furnish an early Semitic example of that 'ritual drama' (*sacer ludus*) which certain scholars have postulated as the true origin of Greek drama. All the elements (viz. the Combat, the Triumph, the Sacred Marriage, etc.) which have been reconstructed by Gilbert Murray, Jane

Harrison, F. M. Cornford and others as essential constituents of the Urform are here indeed present, so that a theory condemned in more 'orthodox' circles as fanciful now demands renewed consideration. If this new view is correct, the Ras Shamra texts will take their place in literature as the earliest examples of European drama and they will, incidentally, throw a flood of light upon the origins of the mummers' play which may then be regarded as the lingering survival of this pristine form.

In concluding this survey, one point should especially be stressed, and that is that all results are necessarily provisional. Much that was written in the excitement and imperfect knowledge of the first discovery is already antiquated, and there is scarcely one scholar who is engaged seriously in this branch of study who would not now wish to forget some of his earlier pronouncements.

Another point that should also be made, and this very strongly, is that for the full appreciation of this literature it is necessary that the services of other than purely Semitic specialists be recruited. There is a danger that material which can only be assessed properly through the fullest co-operation of archaeologist, philologist, folklorist, anthropologist and Classical scholar will suffer through too narrow an interpretation and that wider views will be refused a proper hearing through lack of a duly qualified audience which might judge them.

Modern Views on the Italian Terremare

by D. RANDALL-MACIVER

THERE has been for a good many years an uneasy sense that all was not well with the theory of the Italian terremare as propounded by the original explorers. Writers of general works have continued to accept and to pass it on, in perfectly good faith, as if it were a coin of full value, worth the same now that it was thirty years ago. But those who had the best opportunities of knowing were aware that there had been a very serious depreciation. It was strongly suspected that, like the dollar, it had sunk to at least 59 per cent of its nominal value; but there seemed to be no one who was at once willing and able to state the exact degree of devaluation. Pigorini's staunchest followers, of whom the last survivor died a few years ago, began to admit that there was a considerable percentage of loss; while more impartial outsiders wondered whether the terremare coin had not become totally valueless.

It was a long while before suggestions of this kind were made quite openly. A certain conservatism, coupled with a proper respect for great names and a great tradition, deterred scholars of my generation from being too abrupt or too brutal in breaking with the past. But now the time has certainly arrived for a frank revaluation, and it is perhaps better that this should be made by one who has viewed and sympathized with the whole development of the last half-century, rather than by some more unsparing iconoclast of a younger school. In this latter class I may without offence venture to include Sâflund, a Swedish scholar whose recently published volume¹ has impelled me to put these remarks on paper.

The theory which I am discussing is no doubt familiar in outline to most readers of *ANTIQUITY*. Few of these however will have at their disposal a complete file of the *Bullettino di paletnologia*, in which Pigorini at frequent intervals from 1875 onwards expressed his developing opinions. The Terremare are well described by Peet in his still

¹ G. Sâflund, *Le Terremare*, a work written in excellent Italian and brought out as one of the publications of the Swedish Institute in Rome; published by Gleerup (Lund) and Harrassowitz (Leipzig), 1939. To be reviewed later.

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authoritative work (*The Stone and Bronze Ages in Italy and Sicily*, Oxford, 1909), but the whole doctrine of Pigorini is given in its clearest and most orthodox form by Modestov in his *Introduction à l'histoire romaine* (Paris, 1907). Modestov accepts Pigorini's views as regards the Bronze Age and supports them with an eloquence which is very impressive. It is only at the beginning of the Iron Age that he parts company with Pigorini and refuses to accept the genealogy which would derive Villanovans from Terramaricoli.² In my own first volume on the Iron Age (*Villanovans and Early Etruscans*, Oxford, 1924) I made a similar break with tradition; and it is symptomatic of a modification in their point of view that the same Italian scholars who fiercely protested against Modestov received my work without protest and even with approval. Opinion was clearly changing fifteen or twenty years ago; the change has been still more marked since then. Now it is not merely the validity of the genealogical tree of the Roman which is attacked, it is the whole theory including the conception of the *Terremare* as the central pillar of Bronze Age culture in Italy. It will be well to give in a few lines a résumé of the Pigorini doctrine. At the end of the Stone Age Italy is found to be inhabited by a homogeneous people of Mediterranean origin, living in round huts of wattle and mud and using the rite of inhumation for the burial of the dead. This homogeneity is broken by the arrival of lake-dwellers, who make pile-dwellings, like those of Switzerland and Austria, in all the lakes of northern Italy from Lombardy in the west to Venetia in the east. Their civilization is at the chalcolithic stage; bronze is beginning to make its first appearance but is still rare. The lake-dwellers are succeeded, but only in the eastern part of the above-mentioned area, by an apparently new people who have the curious habit of building pile-dwellings on dry land, sometimes even on hills. Their burial custom is cremation, so that they are sharply distinguished both by their habits of life and by their religious practices from the Mediterranean aborigines. It is inferred that they are immigrants or invaders from beyond the Alps. Their civilization is characterized by a free use of cast bronze, and it is maintained that they were the pioneers of bronze-working in the peninsula. From them, it is said, the knowledge of this craft spread all over Italy. At the close of the Bronze Age their settlements, which were in the eastern valley of the Po, especially round Parma Reggio and Modena,

² *Terramara* is a word coined from the dialect form *terra marna* ('dark earth'). As it is derived from two separate words the correct plural is *Terremare*. The inhabitants of *terremare* are called in archaeological jargon *Terramaricoli*.

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disappear. It is regarded as uncertain whether they were burned by themselves or by their enemies; Modestov holds that they were destroyed by new invaders. Next—and this is the most sacrosanct part of the theory—the Terramaricoli leaving their homes made a long trek, or perhaps a conquering march, which brought them by devious routes to the lower Tiber. In these new surroundings they founded new settlements, no longer in the style of pile-dwellings except for their linear town-plan, in Tuscany and on the Alban hills. Moving down from the Alban hills they founded Rome with a ground-plan like that of a *terramara*, and became the ancestors of the Romans. Finally their civilization, which by this time had reached the stage of the Early Iron Age, developed into what we recognize as Villanovan, and the Villanovan in turn became a powerful influence spreading over every part of Italy.

Modestov, as I have already said, dissents from this last extension and refuses to identify Villanovans with Terramaricoli, but up to and including the founding of Rome he faithfully accepts the whole doctrine. I, fifteen years ago, rejected the pedigree deriving Romans and Villanovans from the Terremare; but it was not then part of my plan to discuss the earlier period, which belongs to the Bronze Age. I have now therefore to say how much, if any, of the first part of the Pigorini theory should, in my opinion, be retained.

First of all then it has long been recognized that the evidence for the supposed trek from the Po valley is of very flimsy character. I have no space to discuss it, so that the reader if he thinks it worth while must test the argument for himself. He must go behind Colini's ingenious pleading and Modestov's eloquence and ardour to examine the actual evidence, which they have in perfect honesty put forward. If he is then convinced I have no more to say on that point; to me and to many others, in Italy and outside, this march has never seemed to be anything but a poetic figment. The entire Pigorini theory indeed was never more than a semi-prophetic forecast, an ideal epic more in the spirit of Vergil than of sober archaeology. To turn it into serious truth discoveries were needed which Pigorini confidently anticipated, but which never occurred. Colini made a gallant attempt to supply some of the most obviously needed links. But he did not succeed and the whole course of subsequent exploration has weakened instead of strengthening the chain. Like many others therefore I reject all idea either of a trek or a conquering march from the valley of the Po to Rome. I have long ago rejected the chimerical idea that this supposed march was prolonged to Taranto.

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How much importance then, if any, can we still attach to the Terremare when so much of the original theory has been abandoned? Are they anything more than a curious local phenomenon? To use the language of my simile has the coin been devalued even far below 50 per cent? I would answer that there may still be a small but valuable kernel of truth underneath all the covering of incredible hypotheses. There is still a good deal of reason for supposing that it was the people of the Terremare who introduced the knowledge of bronze-working into North Italy. This view is in no way invalidated by the new importance which Rellini's discoveries have given to the *extra-terramaricoli*, that is to say the contemporary natives of Italy who lived in wattle-huts instead of in pile-dwellings. For the Bronze Age of Italy, at least in the north, is essentially continental and Central European in character; it is not directly derived from the Aegean. The people of the Terremare may well have been its pioneers. For the present I at least should be willing to retain this not inconsiderable fragment of the Terremare saga.

Mapungubwe

I. THE EXCAVATIONS AND CULTURE

by G. CATON-THOMPSON

THE story of the discovery of Mapungubwe,¹ which led to its systematic excavation, makes reading as vivid and exciting as any adventure-fiction of our youth.

The site is one of many precipitous sandstone kopjes which rear their almost perpendicular sides some 200 ft. above a valley-floor tributary to the Limpopo, which lies only a mile distant, at the point where the Shashi river, flowing from Rhodesia, joins forces with it. Messina—archaeologically famous for its pre-European copper mines—lies about 50 miles west, and it overlooks the meeting point of Transvaal, Bechuanaland and Southern Rhodesian territory. Fortunately for science, British administration being parsimonious in these matters, the hill lay within the Transvaal border.

The region at the time of the discovery, in the New Year of 1933, was one of exceptional isolation; malaria-riddled in summer, the farms were mainly unoccupied, or used for winter big-game shooting. Lion and elephant abound.

The local natives in language and tradition show a mixture of Shona, Venda and Sotho elements. To them Mapungubwe—the Hill of Jackals—was a place of terror and taboo. To climb it was death; even in speech it was safer to turn away and not to point. It was however not inviolate. A prospecting party had climbed it in 1929, and Frobenius is believed also to have visited it that year, but, as usual, left no record.

But these explorers had not seen the importance of the site. Some half century ago a wild white man ('Der Wilde Lotrie') lived in half-crazy solitude in a nearby cave. He climbed the hill and found things there. From it he brought back a large earthenware jar, finer than anything the natives knew. And in due course this passed into the possession of a native who, by then very old, related in 1932 this strange tale of the long past to Mr E. S. J. van Graan, who farms some 50 miles

¹ MAPUNGUBWE: ANCIENT BANTU CIVILISATION ON THE LIMPOPO. Reports on Excavations from February 1933 to June 1935. Edited by Léo Fouché. Cambridge University Press, 1937. pp. XIII, 178, and 45 plates, with maps and diagrams, £2 10s od.

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away. Mr van Graan followed the story up energetically ; and aided by a reluctant and terrified native, unable, notwithstanding, to resist the pressure put upon him, and the lure of a reward, identified the hill ; and with his son and three accomplices found a secret ascent up its cliffs, and forced the passage.

We may at this point hope that the native and his tribe have been very handsomely rewarded, since the intrinsic value of the site has been so rich. This we are not told ; but on plate v appears a picture of Chief Tshiwana, successor and reputed descendant of the legendary chief Mapungubwe, clad in the usual disgusting rags of European discards—a pathetic picture.

The hill-top proved to be a stronghold reached by a cleft made scaleable of old by rock-cut notches. Breast-works of dry masonry crowned the summit, and boulders ready to be hurled upon unwelcome visitors. The flattened top measured 300 yards by 40 wide, and bore an immense overload of earth and midden dust and stone, brought up by hand over a long period to form the huts and hearths, floors and granary-platforms of permanent settlement. Potsherds lay everywhere and search in the loose soil yielded the explorers iron tools and copper wire and gold . . . gold the bane of archaeology, responsible for more pitiable destruction of historical evidence than ever followed in the wake of savage armies. Here the vile metal consisted of thin gold plates beaten into sheathing for carved figurines of rhinoceros and other objects, fixed with small gold tacks ; of bangles, beads. And, more important, iron and a fine pottery bowl. These lay with burial remains which, in the circumstances of the excavation, did not survive intact.

Up to this point the proceedings had pursued a well-worn course, known only too painfully by those who followed the archaeological track of destruction left by Hall and Neal and the pioneer tragedians of the Rhodesian ruins. But imperceptible as it sometimes seems, particularly in official circles, the world since those days has become increasingly science-conscious ; the labours of the teaching profession and those who lecture have not been wholly unavailing.

How Mapungubwe was thus miraculously saved from secret destruction, is best told in the words of the editor Professor Fouché, inevitably condensed.

‘ The five fossickers had realised the schoolboy’s dream—they had found hidden treasure. But they were in a dangerous situation ; they were trespassers and in possession of property which the law might regard as stolen. There was much debate as to what to do next. Eventually the two van Graans were forced to agree to a division of

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the spoils on the spot. The gold was roughly divided into five equal portions. This was the most critical moment in the history of Mapungubwe. The site was so remote and lonely that unscrupulous treasure-hunters could easily have ransacked it completely and got away with their loot. Fortunately the van Graans were men of education, and young van Graan when a student at the University of Pretoria, had become interested in the story of Zimbabwe . . . He remembered regrets expressed in lectures on the subject that at Zimbabwe no human remains had as yet been discovered, since such a find might well solve the mystery.² Here at Mapungubwe they had found what looked like Zimbabwe gold in conjunction with a human skeleton. He realised the scientific importance of the discovery and resolved to consult his old professor. Thus it came about that the writer, some weeks later, received from Mr J. C. O. van Graan specimens of the gold plate, bangles and beads found with the skeleton . . . It was necessary that every effort should be made to recover all the contents of the rifled tomb. With the cordial cooperation of the University of Pretoria, the writer was able to organize an expedition in order to recover the treasures already scattered, and to examine the site. The legal problems arising out of the questions of treasure-trove and ownership were very obscure. These were immediately investigated by Advocates A. A. Roberts and E. C. Niemeyer, while search was also made for the owner of the farm on which Mapungubwe is situated. Police headquarters were approached for authority to deal with the possessors of the treasure-trove should necessity arise . . . Sub-Inspector Kruger, C.I.D. accompanied the expedition . . . The discoverers were traced and persuaded to hand over all their finds at a valuation, and further to make over their discoverer's rights to the University. The tact displayed in these delicate negotiations by Mr F. V. Adams places the University and the scientific world under a deep obligation to him. Sub-Inspector Kruger was not required to intervene officially; his presence and moral authority sufficed'.

The passage has been quoted at length because it is a model of vigorous action—alas rare in archaeological emergencies—taken swiftly, comprehensively, inexorably, to save a scientific trust belonging not to South Africa alone but to the world. We feel sure that a non-gold-bearing site of equal scientific importance would have been similarly treated.

Action thus initiated by private enterprise was sustained and developed. The University of Pretoria acquired the excavation rights and manfully shouldered the heavy preliminary expenses; and at Easter 1933, a scientifically selected party went up to Mapungubwe for a week's test, survey and report. The skirts of the hill were found to be rich in material washed down the cliffs; vast settlement-deposits lay in the vicinity. Professor (then Mr) van Riet Lowe, one of the party, in a brief but excellent report, confirmed the scientific value of the site and urged its excavation. The Prime Minister, General Hertzog—an

² The reviewer classes 'mysteries' along with gold, as a pest in archaeology. The Zimbabwe 'mystery' was dispersed some years after its gold (*i.e.* in 1905). Neither may be said to have existed since.

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example yet to be followed by his English counterparts—declared the undertaking 'a matter of national importance', and General Smuts gave it his energetic attention. In June 1933 the Union Government decided to buy the farm on which Mapungubwe lies³ and to support financially the excavations. The University of Pretoria, in recognition of its exertions, was granted a five years' concession.

In London that summer General Smuts discussed with me the implications of the find and the best procedure in regard to it. I urged the competence of South African archaeologists—Mr van Riet Lowe in particular and Mr Neville Jones—to undertake the excavations, at a time when many were discussing more experienced overseas names: and I stressed the ultimate benefit to Union archaeology of building up through field-work its own tradition of responsibility.

Moreover the Limpopo finds seemed to me almost certainly to be another instance of medieval or later native culture, allied to, though not yet proved identical with, the Rhodesian material. But since the unanimous findings of two successive British archaeologists of authority concerning the latter had failed to overcome South African prejudice against acceptance of a native origin and comparatively late date, and had given rise to much abuse and anger, it seemed to me better that the verdict, similarly unacceptable as it was bound to be, should come this time from South Africa itself.

A preliminary test in July 1933 under the direction of Professor Fouché was made and 23 ft. of occupational débris found to overlie bedrock. An adjacent site, Bambandyanalo, some 500 yards south, later to become of special importance, was discovered, and a general air survey instituted.

Mapungubwe thus saved was now ready for systematic excavation. This began in May 1934 and continued for two months under the direction of Mr Neville Jones and Mr J. F. Schofield, with Mr van Tonder as 'non-technical assistant'. The third and last month was spent prospecting ruins in the neighbourhood. Meanwhile Professor Lestrade, whose ethnographic studies of the Ba Venda, Suto-Chuana and other tribes confers on his opinion particular authority, undertook the difficult task of investigating the traditional connexions between the local natives and Mapungubwe. His contribution (part VI), though not in many ways conclusive in establishing a direct connexion, reveals the ethnic history of the area as a complex affair in which all that seems

³ The figure rumoured was a large one (£12,000). Certainly nothing comparable has before been done on this scale to safeguard archaeological field-work.

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certain is that Shona-Venda clans were conquered, possibly 150-200 years ago, by Sotho elements. Venda, Lemba, Shona, Sotho and Thonga-Shangaan individuals were questioned, and their ability to identify and interpret objects from the old site tested; comparisons were made between the ancient article and its modern representative. Beads and pottery clearly show many points of close similarity, though differences, inevitably, also. Professor Lestrade, who accepts the culture of Mapungubwe as 'Bantu' is inclined to attribute much of its inspiration to Lemba influence. He rightly emphasizes that gold was formerly regarded as a metal of special value only in those areas with Arab contacts; and he would ascribe to the Lembas an attenuated Arab strain and cultural tradition, sufficient nevertheless to account for the fine gold-working of the ancient sites. This negro-transmitted Arab influence was the view I expressed eight years ago; and my archaeological observations in the Hadhramaut in 1937-8 have strengthened it. The beads I found there in tombs covering the last few centuries before the Christian era are totally unlike the Zimbabwe-Mapungubwe series, and contradict any attempt which might be made to overdate to that extent the South African sites. But with them were quantities of small copper tacks, indistinguishable I think (without comparison) from those of gold or copper in South African deposits; they point at least to a common technique in metal sheeting which the disparity in age cannot invalidate.

The volume is an unusually composite one, even in these days of team-work, for it includes the finds made before 1934 (part I) and overlaps at the other end (parts VII-IX) Messrs. Jones and Schofield's work, which ended in September 1934, to be continued since by other excavators. A review of Vol. I therefore which represents the start only of continuous work, that has since yielded a large amount of fresh material and information—enveloped if rumour may be trusted in some remarkable theories—is not an easy task, and certain judgments may be revised or modified in the light of the second volume. We know from an article on Mapungubwe by Mr van Riet Lowe in *ANTIQUITY*, September 1936, that Captain Gardner, who succeeded Messrs Jones and Schofield as excavator, believes the Bambandyanalo site to be far older than Mapungubwe with its gold burials, and to be at the start in no way connected culturally with it. We therefore keep a critical watch for any evidence, stratigraphical or cultural, in support of such a view in the present volume.

Detailed information about the dig by Mr Neville Jones, forms

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part II. Five long trenches, off-sets and tests were cut in the hill-top deposits. These are plotted in red on the admirably clear coloured plan made by Mr van Riet Lowe in 1933. The trenches total about 250 yards in length, and though narrow (5 ft.) were unlucky to miss the grave area beside the original van Graan burial, which was found later and cleared by the 'non-technical assistant' Mr van Tonder, pluckily working on alone through the malarial winter of 1934-5, after Messrs. Schofield and Neville Jones had finished. One trench lies only 10 ft. from an outlying grave. This is not to imply that the 7 ft. or so of occupational débris disclosed in these trenches is less scientifically valuable than a cemetery. Indeed it alone may provide the stratigraphical and relative dating evidence essential to the inquiry; and undoubtedly the excavators, faced with an overwhelmingly large choice of productive areas, were right in choosing it, instead of concentrating, as less scientifically restrained workers might have been tempted to do, on the discovery of graves. Nevertheless it is extremely regrettable that the investigations of adjacent settlement and graves were not both handled by the same excavators. Not only would it have provided Messrs. Neville Jones and Schofield with a wider basis for their conclusions, and comparative data for their Bambandyanalo burials, but it would have averted the misfortune, as it seems to me, of separating in publication the two aspects of interrelated evidence.

In this first volume we are given a long and startling anatomical section (part VIII) from Dr Galloway on eleven of the burials (including the van Graan find, M.I), but there is no corresponding archaeological description or plan, grave by grave, of these burials. All one can deduce after laborious search in several sections, is that three of them (additional to M.I) contained a considerable amount of gold. Some of this is reproduced in three costly colour-plates. We would much rather have had, to complete Dr Galloway's physical analysis, the excavator's detailed report, accompanied by line or photographic plates of the grave-goods, and telling us, what we *must* know, *what sort of deposit the graves were sunk in and covered with?* occupational or not? We shall, of course, expect these details in the next volume.⁴

To return however to consideration of what we *have* been told, we find that Mapungubwe hill-top, thus extensively trenched and admirably documented with cross-sections, yielded thick kraal strata with 'cement' hut-floors from which iron tools and weapons; copper wire

⁴ That the material was occupational is stated by Mr Schofield in a paper dated 1938. *Transactions of the Royal Society of South Africa*, xxvi, part IV.

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glass bead (barely mentioned in this report) has turned up there in quantities, and was present too with a burial in the Mapungubwe grave area. Its importance is manifest, for the moulds in which it was locally made by reusing apparently the small blue cane beads have been found.⁵ It will be of interest to see how the next official publication interprets this discovery in terms of relative dating.

The present bead section would have greatly benefited had it been possible to distinguish, not only which beads came from which site, but at which level at that site: through lack of that facility for which the excavators are probably not responsible critical analysis is defective. A useful list of beads found at bedrock on Mapungubwe hill is however given.

The beautifully illustrated publication of the pottery (part III) by Mr J. F. Schofield is a masterly contribution to African archaeology in general and South African in particular. It is logical in presentment, exact in description, and legitimately bold in deduction: it sets a standard for South African wares well up to the exacting level of modern professional archaeology.

In a short introduction Mr Schofield's general attitude to his subject is set out, with emphasis on the continuity of traditional technique and pattern in pottery making amongst the Bantu; since women are the potters who, when one tribe overruns another, will mingle in their craft the newly-acquired tradition in their old.

Despite the admonitions of the physical anthropologist he uses the word 'Bantu' in a cultural sense to embrace his materials; and rightly in my opinion, for a certain underlying unity of social and material culture, superficially diversified though it be, is exhibited by different tribal speakers of the Bantu tongues. 'Women make the pots as well as teach the babies to talk', as Professor Myres once wittily put it in a contribution to this subject. The word thus used in a combined linguistic and culture-complex sense, is surely no more objectionable than the word 'Arab', which includes similarly diverse ethnic types, held together on the material and psychological plane by a fundamental continuum of thought and culture.

The pottery, then, of Mapungubwe and Bambandyanalo is 'Bantu', and has affinities with modern Bantu, not with Hottentot or Bushman, wares. Further, though more abundant and perhaps consequently more varied, much of it has a very close resemblance 'almost amounting

⁵ *Bantu Studies*, December 1937. 'Beads of the Water' by C. van Riet Lowe.

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to identity' to what has been found at Zimbabwe and other Rhodesian ruins, and which lately has been recognized in several other places in Southern Rhodesia.⁶ After close attention to the nineteen excellent plates of drawings, I concur.

An important classification of the wares into three main groups follows. M1 is a fine ware usually black to grey with or without wet-incised decoration of hatched triangles or other geometric figures. The forms are restricted to simple bowls of four main types, and shouldered pots with high necks. One would however like to know in what proportion red and black wares of class M1 were found, particularly as our curiosity is aroused by a colour-plate of a lovely shallow bowl with haematite burnish and ledge-lugs found with a burial (no. 11) on Mapungubwe hill which Dr Galloway, although the bones are evidently fragmentary, pronounces confidently to be Bush or Boskopoid.

This M1 ware, ranges back to bedrock level on Mapungubwe hill and was found also at Bambandyanalo midden, but if at bedrock is not stated. It is the B and B2 of my Zimbabwe classification⁷ as far as the black ware is concerned.

M2 is a mainly coarse brown ware, burnished or matt. Diagonal or hatched bands are roughly made with comb, wire bangle, triangular stylus or other instrument. Some decorations are scratched after firing. The forms are varied, and include spouts, pedestals, ring-bases and plastic knobs and ornament. The ware is associated with M1 at bedrock level on the Hill, and at Bambandyanalo becomes the ascendant class. At Zimbabwe my class A is certainly included in this more comprehensive gathering.⁸

Class M3 includes provisionally all pottery not apparently of local fabric, but derived from other tribal elements. It includes the polychrome ware of Khami and other Rhodesian ruins—my type D.

Mr Schofield's conclusions, based on a quantity of carefully sifted evidence from native wares past and present, and supported on more

⁶ For instance J. F. Schofield, 'Salisbury Commonage Sites'. *S. African Journ. Sci.*, vol. XXIX, 1932. 'Pottery from the Salisbury District', *Trans. R. Soc. of S. Africa*, vol. XXVI, part IV, 1938. Also from well-stratified deposits at Gokomere.

⁷ It lay *in situ* beneath the foundations of the Conical Tower; in the Acropolis midden on bedrock, 24 ft. below the surface; and in rare scraps (five only) below the cement floors of the Maund ruins (all at Zimbabwe) but was plentiful above them.

⁸ *Zimbabwe Culture*, 1931, p. 25: 'The vertical distribution is interesting: in the Maund it is almost entirely confined to the sub-pavement stratum. Out of 651 sherds . . . only 4 came above the cement floor'.

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general grounds by Mr Neville Jones, are that M1 is of Shona stock ; M2 of Sotho stock, and that both contributed their separate streams to the creation of the Mapungubwe culture, where overlap took place. He suggests that Zimbabwe was founded by people of Sotho stock (pottery A.Z=M2) probably before A.D. 1300, who about 1450 deserted it and crossed the Limpopo, to establish themselves around the foot of Mapungubwe kopje. At Zimbabwe they were succeeded by people of Shona stock (pottery B.Z and B2=M1) who continued in occupation until the eighteenth century, spreading the while into the Northern Transvaal, to which they brought their goldsmith's crafts, and, on Mapungubwe hill-top, amalgamated with their Sotho predecessors. Both peoples were cattle herders *and* agriculturalists. A close connexion between the Zimbabwe culture (or cultures) and the two Limpopo cultures is indicated, supported, unanswerably, by pottery, beads, copper, bronze and iron articles, gold beads, gold sheeting, gold tacks.

The validity of this view, to which Professor Lestrade's inquiry amongst the local natives raises no objection, is seriously challenged in the skeletal analysis by Dr Galloway, a member of Professor Dart's staff at Johannesburg.⁹ He identifies from the fragmentary material dealt with in this volume, a new non-negro race to which he gives the compound name Bush-Boskop—unfortunately in my opinion, since Boskop man is a unique fossil almost certainly of stone-age date, and confusion may result. A complete dead-lock between the physical anthropologist and the massed evidence of cultural anthropology seems thus indicated ; and neither is likely to recede. The problem embraces also the Rhodesian ruins and ancient mines, for there though scanty, physical material is not entirely absent. Such as it is it had the benefit of the late Dr F. C. Shrubbsall's examination : though intimately acquainted with Bush and Hottentot material, he pronounced it, in each case, to be negro. Other remains from ancient mine-workings and two imperfect skulls from my Dhlo-Dhlo excavations, were considered to be negro also by Sir Arthur Keith, who stated : ' thus all the remains we have from ancient ruins and mines are of the negro (or Bantu) type. No trace of Arab, Egyptian, Bushman or Hottentot '.

Personally I should find it hard to believe, without overwhelming proof unlikely to be found, that a Khoisan people, plus or minus Boskop, evolved the Mapungubwe culture—it is alien to all known tradition of

⁹ Dr Morant's review appended has not been seen by me ; nor my contribution by him. Professor Dart himself has always hotly maintained his belief in the non-negro origin and very ancient date of the Rhodesian ruins.

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these peoples. Nor is a problem-evading assumption of Bantu culture-adoption any more convincing. I venture to think that a solution acceptable to anatomist, archaeologist and anthropologist will not be found until far more work has been done on an extensive scale, not only on the physical characteristics of Bantu-speaking tribes 'that congeries of Hamitic-Negro peoples, gathered together in the scientific net of linguistics, but escaping capture through its wide mesh which seeks to draw them on board for closer racial dissection' ;¹⁰ but also on a far longer series of human remains from Rhodesian, Transvaal or other sites, yielding the culture of this very remarkable ancient civilization of South Africa.

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II. THE SKELETAL REMAINS

by G. M. MORANT

PART VIII (pp. 127-174) of the volume reviewed* has this title. It was written by Dr A. Galloway and he acknowledges help received from seven research workers in the Department of Anatomy, University of the Witwatersrand, who had previously collaborated with him in an anthropological report on 'Bush' skeletons. The conclusion reached then is that 'generically, peoples called Bushmen, Hottentots and Strandlopers are one', and the term Bush Race is applied to these groups. The work on the new material is remarkable on account of the thoroughness with which it was carried out, and it is also noteworthy because the conclusions reached appear to conflict with those formulated by the archaeologists.

Unfortunately, it cannot be said that the human remains which have received so much attention form an extensive series. They consist of fragments of the skeletons of eleven individuals, excluding a mandible of uncertain date which is said to be 'predominantly Negro'. One

¹⁰ *Zimbabwe Culture* p. 8. Is it not possible that unsuspected Bush features lurk in the physical make-up of certain 'Bantu' tribes? Dr Galloway finds, as we all do, what he is on the look-out for, and has reported 'Bush-Boskopoids' from ancient pit-circles at Penhalonga, Southern Rhodesia (*S. African Journ. Sci.* xxxiii, March 1937). Now Dr Wells, from the same School of Anatomy at Johannesburg, reports them in the Mumbwa cave, Northern Rhodesia, in association with pottery, iron and late stone age implements. (*Man*, May 1939).

* See footnote, p. 324.

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of the eleven is represented by bones which are so fragmentary that it was decided that no description would serve any purpose. Of the remaining ten individuals, six are represented by skulls (with or without a few vertebrae) and all except one of these are very imperfect, while four people are represented by fragments of the skull and other parts of the skeleton. Six are adult—three female, one male and two unsexed owing to defect—and four immature. It is clear that the bones from Mapungubwe are in a very bad condition. The material was reconstructed to a considerable extent in the laboratory.

Given such a series, most anthropologists would decide that there could be no hope of deducing useful conclusions regarding racial affinities from any of the specimens other than the crania, while the crania could not be expected to give any but very tentative results. Dr Galloway, however, has no hesitation in deducing very far-reaching conclusions from the restricted material. He provides a detailed description of each specimen and gives its racial pedigree without hesitation. The first, for example, is said to be definitely 'a Boskopoid type with some Bush features and no Negro features'; the third may 'be diagnosed as a Bush-Boskopoid admixture with a slight Negro infiltration, if features of the Negro adult can be regarded as Negro in an immature subject', and so on in each case. Tables of measurements are also provided, but these are considered to be of lesser importance. For example, the best preserved part of the third skeleton, which is that of a child about 10 to 12 years old, is the skull, and this is 'so distorted and warped that measurements have no significance, so diagnosis must be based on non-metrical features'.

The descriptions of the bones given relate principally to morphological features of this kind. It is misleading to some extent to call them non-metrical, since many refer to the sizes or shapes of particular parts; hence they might be expressed by measurements, though it would doubtless be difficult to devise effective ways of expressing some of the features metrically. One objection to verbal descriptions of skeletons made for anthropological purposes is that they are inexact. Anyone wishing to compare the Mapungubwe with another series could not be sure of estimating particular features on precisely the same scales, as it were, as those used by the workers in Johannesburg. Measurements obviously have a great advantage in this respect. The remarks on non-metrical features are necessarily subjective in a way that measurements are not, and this is felt particularly when comparisons are made between different specimens. Judgments are then influenced

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by the experience the observer has had in handling the specimens in question and others, and they cannot be adequately controlled by an anthropologist who had not had an opportunity of examining the former.

The study noticed forms part of a comprehensive report on excavated material from a particular site. Archaeologists interested in the new knowledge gained are not likely to read the whole of the section on the skeletal remains. The greater part of it is too detailed, and too burdened with technical terms, to make any appeal to them, and after reading the introduction to the section they will doubtless turn to the discussion and conclusions. These set forth the views of the writer in a concise and straight-forward way, but in fact they take a great deal for granted, viz. the legitimacy of the method of analysis used in the body of the study. The archaeologist can only suppose that this is a highly specialized matter: he may, or may not, be willing to take the analysis for granted and to accept the conclusions presented. The way in which the general question of method is considered by other physical anthropologists may be of some interest to him.

There are two principal ways in which series of bones have been studied for the purpose of disclosing the racial relationships of the people they represent. The first and earlier method is that of the comparative anatomist. He gives verbal descriptions—aided by photographs, drawings and possibly measurements—and his general conclusions will be derived from a comparison of all the principal features of the specimens. It would usually be agreed that this is the best way of deciding whether a particular skeleton represents the modern species of man or not, providing, of course, that the observer had had a sufficiently wide experience of skeletons belonging to this and other species. By merely handling the bones the anthropologist may be able to decide such a question to his own satisfaction, and he may be able to convince his colleagues by a verbal account of the comparisons made, though this is not likely to convey much to anyone who has not handled the material. The experienced observer will be able to appreciate whether all the characters—considered singly or in conjunction—of the new skeleton fall within the range of variation exhibited by modern man, or whether some of its characters fall outside this range, and thus indicate distinction from *Homo sapiens*. An examination of this kind is far more difficult to carry out satisfactorily when it is a question of distinguishing two very similar groups of individuals. There may be no anatomical features which distinguish all members of Group A from all members of Group B: their distributions may overlap for all characters, so that

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the distinction depends on small differences between average values. There is every reason to believe that this is the situation found in the comparison of all pairs of races of modern man. There are no known osteological characters for them which make complete separations such as those made by skin colour in the case of certain groups of living people.

The second, and much more recent, method of analysing osteological material depends on metrical descriptions. It became customary for anthropologists to record measurements of bones nearly a century ago, and the records are now quite extensive: they relate far more to the skull than to any other part of the skeleton. Measurements of large numbers of crania are only available for a particular group of characters which aim at giving a description of the sizes and shapes of the skeleton of the head considered as a whole and of all its principal parts. They are necessarily restricted in scope and they give no information regarding many anatomical details. On the other hand, the measurements have several advantages. They are objective records: if taken accurately and in standardized ways, they record exact information which does not depend on the experience of the observer, and hence they can be used precisely for comparative purposes by other workers. Furthermore, the measurements make it possible to obtain reliable estimates of certain characteristics of the populations represented, such as average values and measures of variability, which cannot be obtained in any other way with anything approaching the same degree of precision. The ways in which the metrical data may be used to give information of this kind were not fully appreciated until modern statistical methods had been applied to them. Karl Pearson was the pioneer in this field. A new technique of description and analysis was slowly evolved and, though it cannot be said that the form of this technique has been definitely established yet, still the value of the new approach is sufficiently evidenced by the fact that a majority of the researches in physical anthropology published in recent years employ statistical methods.

Anthropologists must not be divided into those who use anatomical and those who use metrical methods of comparison. In fact nearly all use both methods to some extent, though most definitely incline in the one direction or in the other. As there are two ways of investigating the same problems, it is to be hoped, of course, that they will lead to concordant results: they should be complementary and in no sense antagonistic. Unfortunately, it is quite impossible to reconcile some of the

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general conclusions reached by statistical means with conclusions of certain kinds regarding relationships put forward by those who rely almost entirely on anatomical comparisons. For example, Dr Galloway implies that it is possible to assign any individual skull to a particular racial group, and his diagnosis is based almost entirely on 'non-metrical' features. On the other hand, the biometrician asserts that it is usually quite impossible to do this by using the commonly recorded measurements, and that it is unlikely that any others which might be taken, if records as extensive were available for them, would make it much easier to determine the race of an individual specimen. In fact he goes much further than this by declaring that 50 or more adult skulls (not in a fragmentary condition) of one sex must be available before it becomes possible to distinguish with any assurance the group they represent from closely related groups. One advantage of statistical analysis is that an estimate can be obtained of the amount of information necessary to give a reliable conclusion of the kind required. There is no such control in the case of the anatomical analysis, and no guarantee that two workers using such a method will reach the same results.

Anthropology is the study of large groups of people, and statistics is the science concerned with the reduction and comparison of group data in a systematic way. One of the most important characteristics of a population concerns its variation, that is to say the extent to which different individuals composing it differ from one another. The statistical treatment of measurements for them makes it possible to measure this feature, and information so obtained regarding racial communities has led to certain definite conclusions which are not generally appreciated. Numerous adequately long series of skulls of various dates, coming from all parts of the world, have been studied, the earliest as yet available being the predynastic Egyptian from Badari. These may be chosen, by taking into account any relevant archaeological or other evidence, so that each represents a single community, as far as can be told. Each may be believed to represent a population such that its members—or the majority of them, at least—had been intermarrying for a number of generations. All series so chosen exhibit very considerable variation, and the majority of them show a remarkably close approach to equality in this respect. The variabilities of a few of the series are found to be exceptionally large, and in these cases it may be supposed that heterogeneous groups are represented. In general it may be concluded that there is no evidence to show that racial communities have become appreciably more variable

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in the past 7000 years. The modern populations of Europe show a tendency to be slightly more variable than those of the continent in late prehistoric and historic times, and also slightly more variable than modern populations in other parts of the world, but no stress can be laid on these distinctions. Anglo-Saxons differed almost as much from one another as seventeenth century Londoners did.

When ways of analysing anthropological material are under review, considerations of this kind are of the utmost importance. One great disadvantage of verbal descriptions of bones is that they convey no precise impression of the variability exhibited by a series in the case of the features treated. Descriptions of 'non-metrical' features are inadequate in this respect, and it may be argued that those who rely on them in making racial comparisons fail to appreciate the importance of a knowledge of the natural variation exhibited by the groups to which reference is made. This seems to be obviously so when modern series are interpreted in terms of descent from palaeolithic populations represented by single specimens. Dr Galloway refers frequently to the type of Boskop man, and his racial diagnoses of individual skulls or skeletons include such labels as Boskop-Bush, Boskop-Bantu, and Bantu-Bush-Boskop. But Boskop man is known only from a very imperfect skull which lacks the facial skeleton. It is very unlikely that he was a perfectly typical member of the group to which he belonged, and our knowledge of the variation exhibited by that group is absolutely nil. We are told that the Matjes River and Zitzikama types are variants of the Boskop, but the evidence is quite insufficient to judge whether these other specimens represent the same population or not. Speculation in terms of the almost-unknown is not likely to be very profitable.

It is most unfortunate that there should be such a great discrepancy between the results obtained by the two different methods of analysis employed by physical anthropologists. The one method leads to definite statements regarding the racial origins of individual skulls or skeletons, and it appears to provide all that the archaeologist could desire. Those who trust in the other method say that it is incredible that so much information can be derived from so little evidence. They should not assert that the anatomical appreciations are erroneous, but they may consider them to be mere speculations which should be recognized as such as long as an effective guarantee of their accuracy is lacking. Some control is obviously needed. On the other hand, the archaeologist is likely to be disappointed if he submits a short series of

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human remains—obtained with great difficulty, perhaps—to an anthropologist who trusts principally in metrical comparisons. If there are fewer than 50 skulls or skeletons, the latter can only say that measurements and descriptive remarks should be recorded, by all means, but that it is unlikely that the short series will lead to any definite conclusions regarding the racial relationships of the group represented. The data given will be of such a nature that it will be possible to pool them with any which may become available later for individuals belonging to the same group. After all, the biometrician may point out, the sample represents a large population made up by some tens of thousands, or even millions, of people, perhaps; and it surely is unreasonable to expect that much information regarding a community of that size can be derived from ten or twenty individuals. The archaeologist must hence expect to get reports of different kinds, according as one species of anthropologist or another examines his skeletal material. In time, no doubt, anthropologists will be able to speak with a single voice.

It is proper that this discursive review of a specialized study should be concluded by a summary of the main conclusions stated in it. The Mapungubwe remains are said to exhibit South African—Bush, Boskop and Negro—traits only, so that there is no evidence of foreign admixture. The skulls are compared especially with a Basuto series representing the modern Negroes of the Transvaal, and it is concluded that the earlier people are 'definitely non-Negroid of the Basuto type, but a stabilized Bush-Boskop group'. At the same time the Mapungubwe series 'represents a homogeneous people, which had been stabilized over many centuries, since the racial features are so constant'. An appendix gives detailed descriptions of the dentitions, and it is concluded that 'all the teeth have a frankly Bush or Boskopoid facies'. The archaeologists conclude that the culture of the people was definitely 'Bantu', however, and this appears to conflict with the evidence of the people themselves, that is to say of their bones. In the present writer's opinion it would be unwise to attach significance to the apparent discrepancy, owing to the paucity of the physical evidence and to the uncertainty attached to the interpretation of it. Much more abundant material would be required to establish such an unexpected conjunction of race and culture.

Notes and News

WATTLED HOUSES IN SCOTLAND

Exact information about houses in the Highland Zone built of other material than stone is not easy to come by. The houses themselves vanish and leave no trace ; whereas the more durable remains of stone houses and huts, however primitive, are for that reason far more common. One is therefore apt to confuse the relative importance of stone and wattle buildings during past ages.

The following account by Arthur Mitchell, in 'Vacation Notes' is, by the kind permission of the Society of Antiquaries of Scotland, reprinted from their Proceedings, 1875, x, 609-11.

' St. Wallach is said to have lived in a poor little house woven together of reeds and wattles, and this seems to have been reckoned a hardship and a mark of humility. We cannot, however, safely infer from this that the people among whom he laboured, and who were neither self-denying nor humble, lived in houses of a different and more comfortable character. Indeed, it seems surprising to find so much made of the saint's wattled dwelling, since houses constructed more or less completely of *wattle and daub* still exist among us ; and I know that they can be tolerably comfortable. At Inverie, in the parish of Glenelg, I once¹ slept in a house the partitions of which were of wattling plastered with clay ; and I find an entry in my journal (16th October 1866), in which I say that in Kintail, Glengarry, Glenmoriston, and Lochaber, "wattled partitions plastered with clay are frequent—outside walls too, but these generally for sheds or byres". Another entry occurs, 17 September 1864, very much to the same effect. Captain Thomas tells me, from personal observation, that wattling was till lately, and probably still is [1875], common in some parts of the Highlands. Nor was it confined to the dwellings of the poor. In Duthil, for instance, the old house of Inverladnan, in which Prince Charles passed a night, had creel partitions ; so had the house of Shillochan in the same

¹ In 1859, or 1860, Mr W. F. Skene, when reading these notes in proof, kindly furnished me with the additional fact that the well-known Macdonell of Glengarry built one of these wattled houses at Inverie, and occupied it as a summer residence for many years. Mr Skene adds that he has often slept in it.

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parish.² At Stronmilchain, in the parish of Glenurchy and Inishail, M'Gregor of Glenstrae had a house built of wattles, said to have been surrounded by a moat, and accessible only by a drawbridge³; and about the end of the eighteenth century, the second son of Cluny, afterwards the Chief, brought home his bride, a daughter of Lochiel, to a wattled house at Nuid, near Kingussie.⁴

'Walker, again, writing between 1760 and 1786, says, "When wood is at hand they erect what is called creel houses. These are formed of wooden posts, interlaced with branches of trees, like wicker-work, and covered on the outside with turf".⁵

'When we hear of persons living in creel houses, it would be entirely wrong to conclude that they are either half-barbarous or peculiarly wretched. The fact may be nothing more than an outcome of circumstances, and no mark at all of inferiority or misery. The early settlers in Australia were persons who had been born and brought up in stone houses, in which not a few of them had lived luxuriously, yet we read that their houses in Melbourne "were most primitive in form and construction", and "were chiefly limited to one storey, and usually built of wattle-stems interlaced, and plastered with mud, known as wattle and daub".⁶ Mr Robert Tennent tells me that he long lived in such a house, of which he has shown me a photograph, and in which he enjoyed no little happiness and comfort.

'Wattle-built churches' were common, and have received notice at the hands of various writers⁸; but in the "Martyrology of Donegal"⁹

² I state this on the authority of Mr David F. Ross. It was in the house of Shillochan that a remarkable piece of Scotch carving on fir was lately discovered. See *Inverness Courier* of 24 September and 15 October 1874.

³ *New Stat. Acct.* vol. VII, p. 97.

⁴ *Book of Deer*, by Dr Stuart, preface, p. CLI.

⁵ Walker's *Econom. Hist. of the Hebrides and Highlands*. Edin. 1806, vol. I, 95.

⁶ *Glimpses of Life in Victoria*. By a Resident. Edin. 1872, p. 10. See also *Voyages to the East Indies*, by John Splinter Stavorinus, Lond. 1798, vol. I, p. 57.

⁷ An oratory built of wood, wattling, or clay was said to be built 'after the manner of the Scots', and one of stone 'after the Roman manner'. The wooden oratory which King Edwin erected at York was enclosed in the stone church which he afterwards built there; and William of Malmesbury tells us that the 'old church of wattled work'—St. Mary of Glastonbury—was, at the end of the sixth century, 'covered with a coating of boards' by order of Paulinus, Bishop of York. (See Babington, *Arch. Camb.* 1857, vol. III, p. 147).

⁸ Dr Stuart, *Book of Deer*, pp. CXLIX to CLVII; R. Perrott in the *Arch. Camb.* 1857, vol. III, p. 147; and Reeve's (*sic*, for Reeves's) *Adamnan*, pp. 106, 114 and 177.

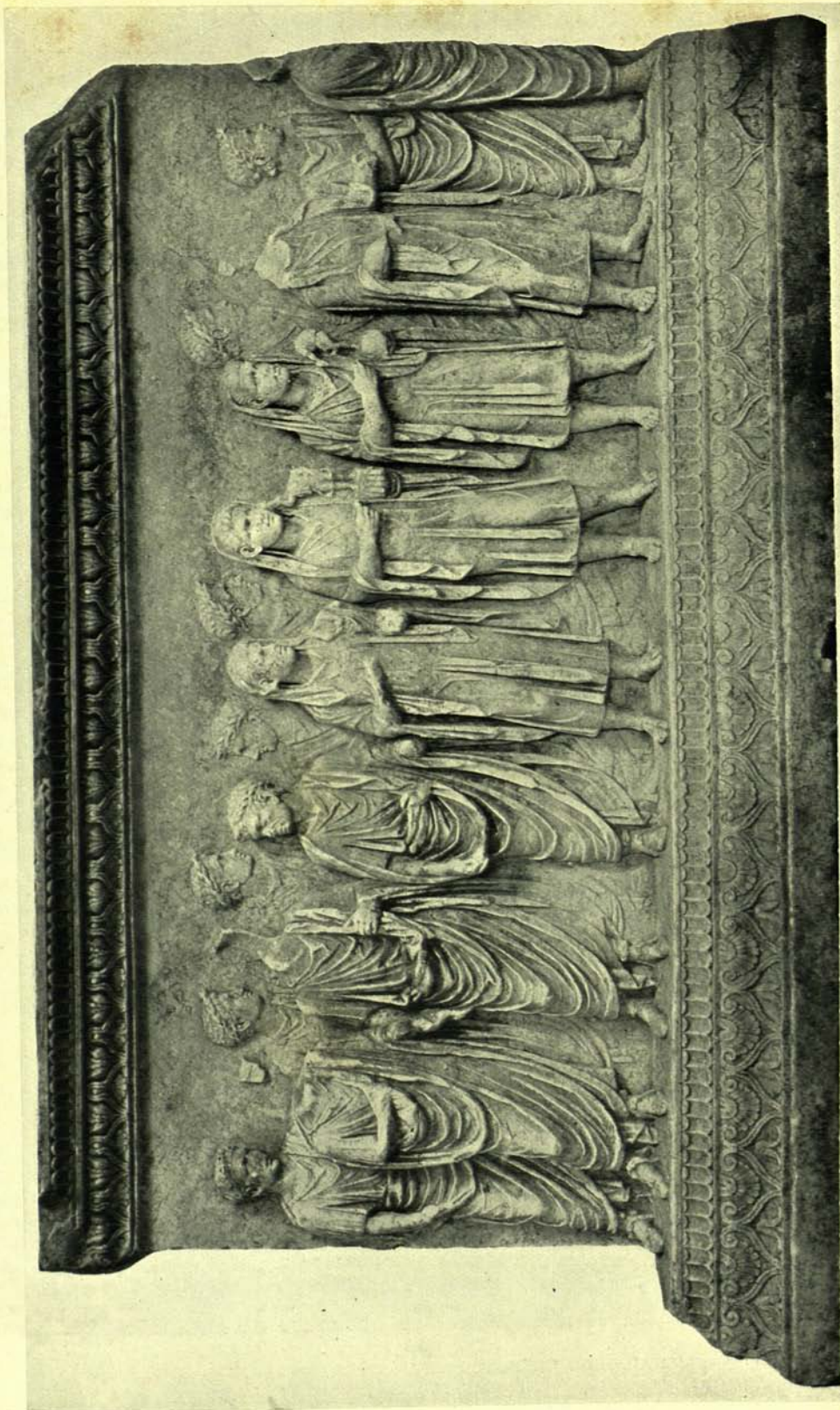
⁹ *op. cit.* p. 177-9.

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there is an interesting reference to one, for which, as for very many other references, I am indebted to Mr Anderson. We are there told that Mochaoi or Caolan, Abbot of n'Aondrium [*sic*, for Nendrum], in Uladh, went with seven score young men to cut wattles to make a church, and that he was himself cutting timber like the rest when a bird, "more beautiful than the birds of the world", spoke with him from a black thorn close by, and continued speaking for 300 years, while the saint slept "a sleep without decay of the body"—his bundle of wattles lying the while by his side, like himself keeping fresh and without decay. Machaoi is said to have been alive in A.D. 496. St. Patrick gave him a crozier, called from the legend of the Wattles the *Baculus Volans*, or Winged Crozier'.

DISCOVERIES IN ROME (PLATES I-II)

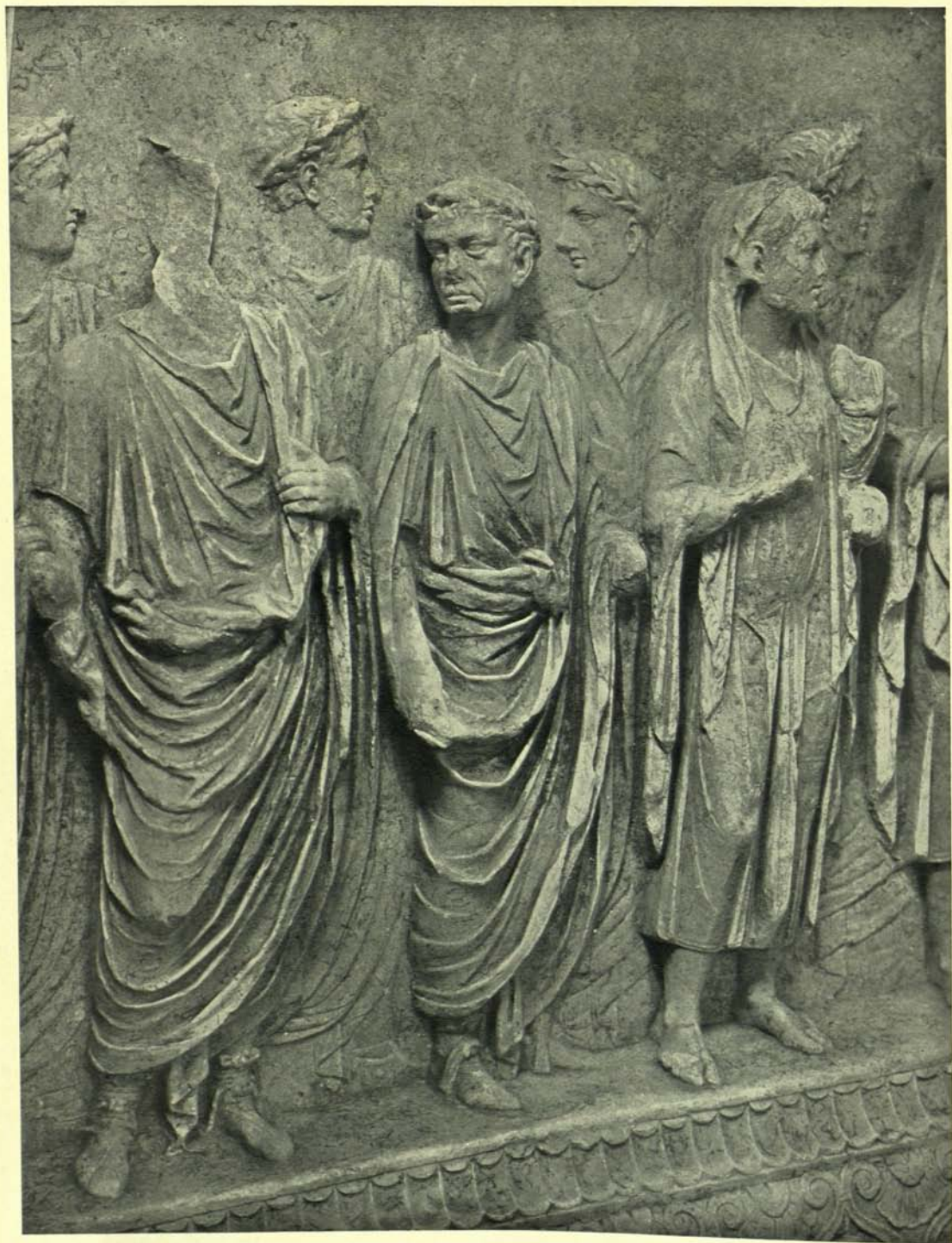
Some of the most important discoveries of recent years in Rome have resulted from excavations in and near the Palace of the Cancelleria. It was in this part of the Campus Martius that the consul Aulus Hirtius received state burial after the siege of Mutina in 44 B.C.; and now a part of his funerary precinct has been found, with two boundary-stones bearing his name. The other objects from these excavations were not *in situ*, hence their interpretation is more open to conjecture. The period of Tiberius or shortly later is represented by a long slab of marble containing most of one face, and the adjoining portion of another, of the frieze of what was presumably a square or rectangular monument; it contains many figures, some of which are represented as holding figurines of Lares. Six other slabs, on a larger scale, in fact over life-size, clearly belonged to a monument of about the year 93 of our era, as they include the representation of the triumphal return of Vespasian and a similar appearance of Domitian. Their surface is almost perfectly preserved; the structure to which they belonged cannot have stood very long before it was dismantled and its sculptured details placed, face inwards, leaning against a wall; it is probable that a flood of the Tiber buried them with alluvial deposit in that position, thus protecting them from deterioration and discovery until the year 1938. Both the Tiberian and the Domitianic slabs are sure to claim a prominent place in future discussions of Roman art: the former are distinctly in the tradition of the long sides of the balustrade of the Ara Pacis Augustae, while the latter anticipate the stylistic qualities of Trajan's Arch at Beneventum. The generosity of Dr Filippo Magi of the Vatican Museum enables us to show, in PLATES I and II, a general



EARLY IMPERIAL FRIEZE FROM THE CANCELLERIA, ROME (see p. 344)

Courtesy of Dr Filippo Magi

PLATE II



DETAIL FROM THE FRIEZE REPRESENTED ON PLATE I
Courtesy of Dr Filippo Magi

PLATE III



CURVED HORN SICKLE FROM KARANOVO, BULGARIA (see p. 348)

PLATE IV



PIRIFORM PITCHER FOUND AT KARANOV (see p. 349)

Height 18.7 cm.; diameter 13.1; mouth 7.5

PLATE V



FIG. 4. SUBMERGED BOAT, YORKSHIRE (see p. 349)
Site of boat at low tide, looking east, and showing western end after excavation. Six feet of the centre plank are shown. The eastern end is marked by a stake in background

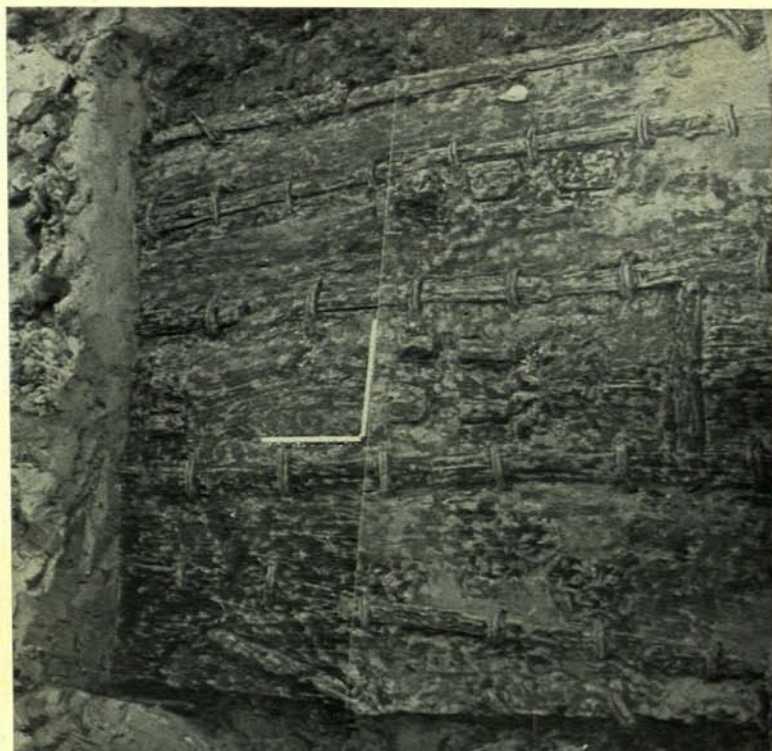


FIG. 5. SUBMERGED BOAT, YORKSHIRE
Five planks in place with battens and ties (each arm of the rule is one foot)
In the foreground is the batten mentioned on page 352

PLATE VI

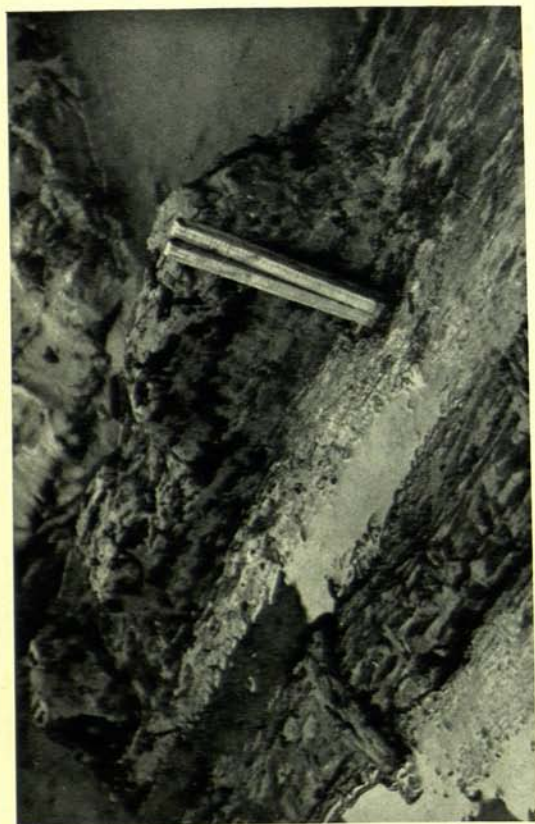


FIG. 6. SUBMERGED BOAT, YORKSHIRE (see p. 352)
Mast step (?) on centre plank near middle of the boat

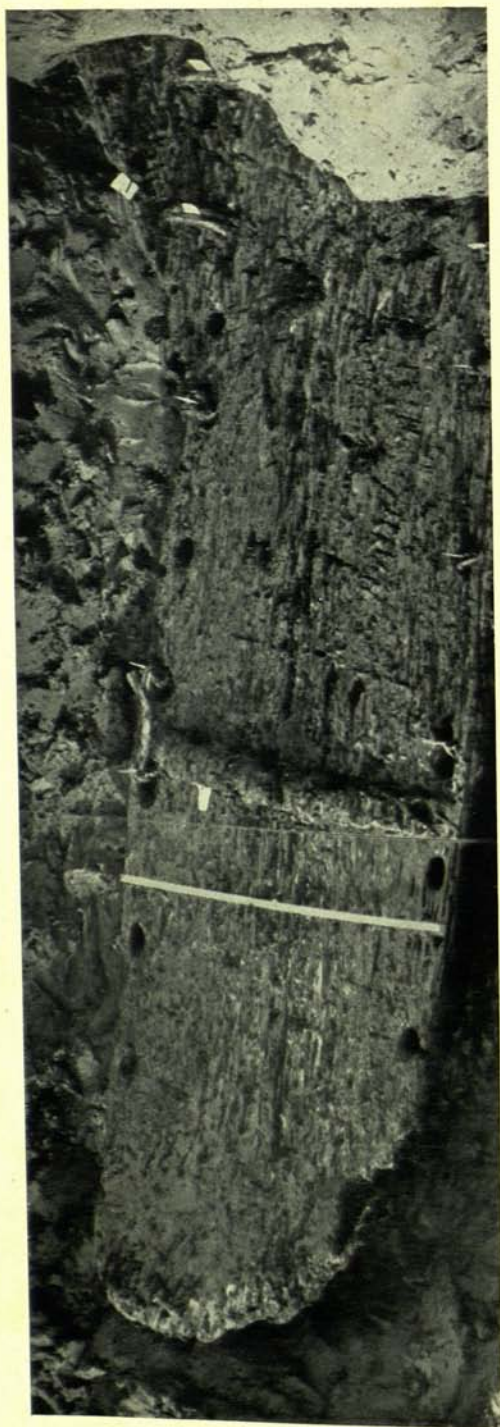
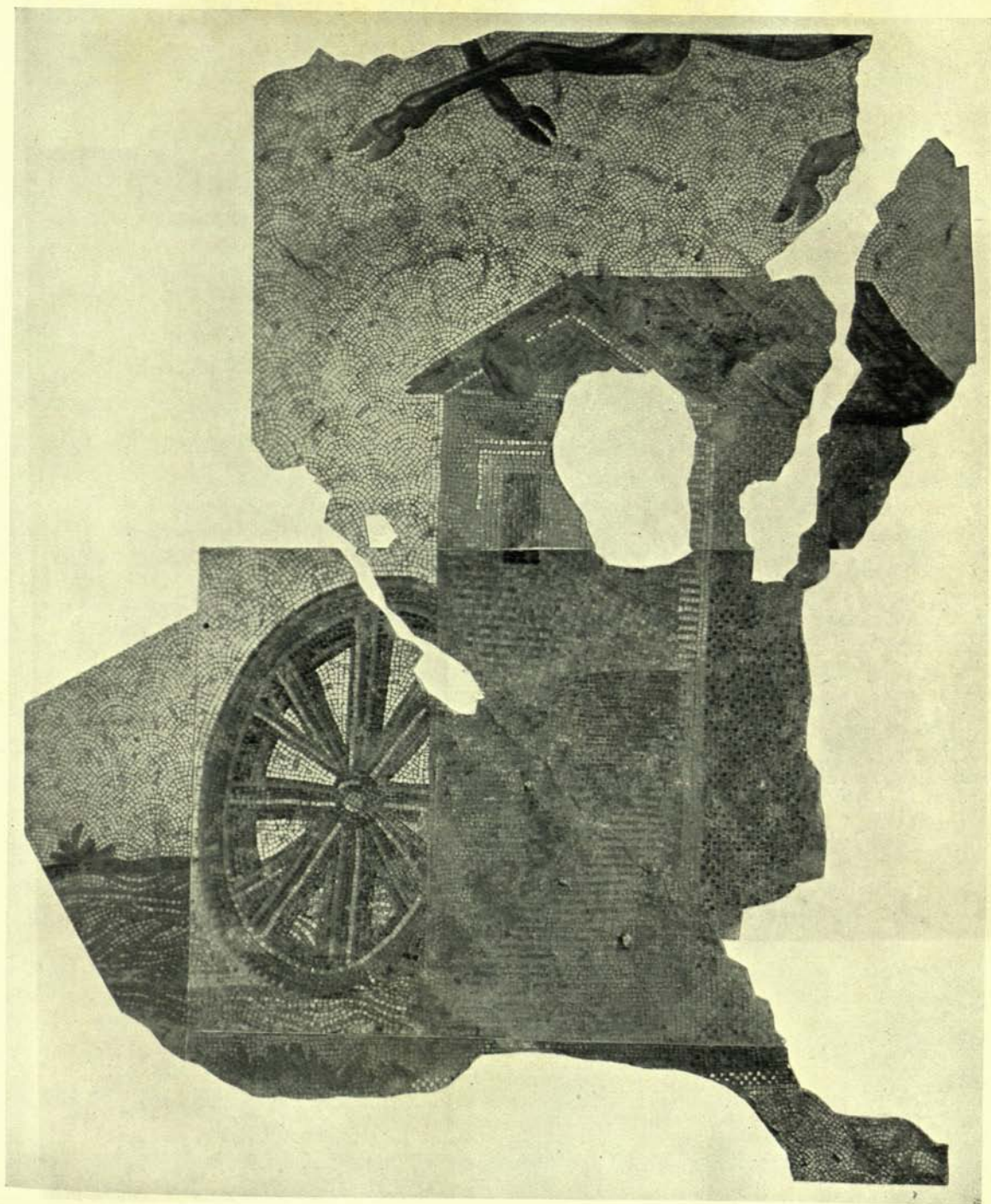


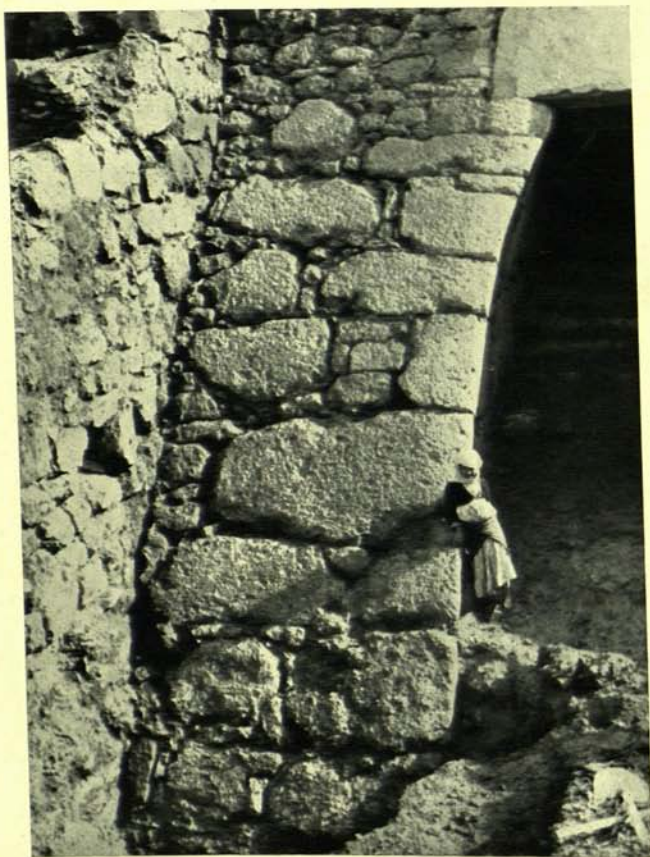
FIG. 7. SUBMERGED BOAT, YORKSHIRE (see p. 352)
Western end of boat, with northern plank beginning (on right), cross ridge, holes for ties and upturned end (rule two feet)

PLATE VII



MOSAIC (5TH CENTURY) OF A ROMAN WATER-WHEEL FROM THE GREAT PALACE OF BYZANTIUM (see p. 354)

PLATE VIII



RAS SHAMRA-UGARIT: POSTERN GATE AT THE FOOT OF THE
STONE GLACIS (*see* p. 358)
Ph. Claude F. A. Schaeffer

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view and a detail of the earlier frieze. All these monuments (except for one of the Domitianic slabs, which was found outside the territory of the Cancelleria and hence belongs to the City of Rome and has been taken to the Antiquarium of the Caelian), together with the epigraphical and religious remains from late antiquity which were discovered in the same excavations, and which considerations of space prevent us from describing, have been taken to the Vatican where they will occupy a place of distinction; Dr Magi's treatment of the friezes when it appears in print will arouse the greatest interest.

We wish to thank Dr A. W. Van Buren, of the American Academy in Rome, for his good offices in this matter and for the above description.

KARANOVO, BULGARIA (PLATES III-IV)

North of the village of Karanovo, in Nova Zagora, southern Bulgaria, is one of the largest mounds in the country, some 12 m. high with a diameter of 110 by 180 m. For years the local villagers have used this mound as a source for fertile earth, and have found various small objects now deposited in Nova Zagora Museum.

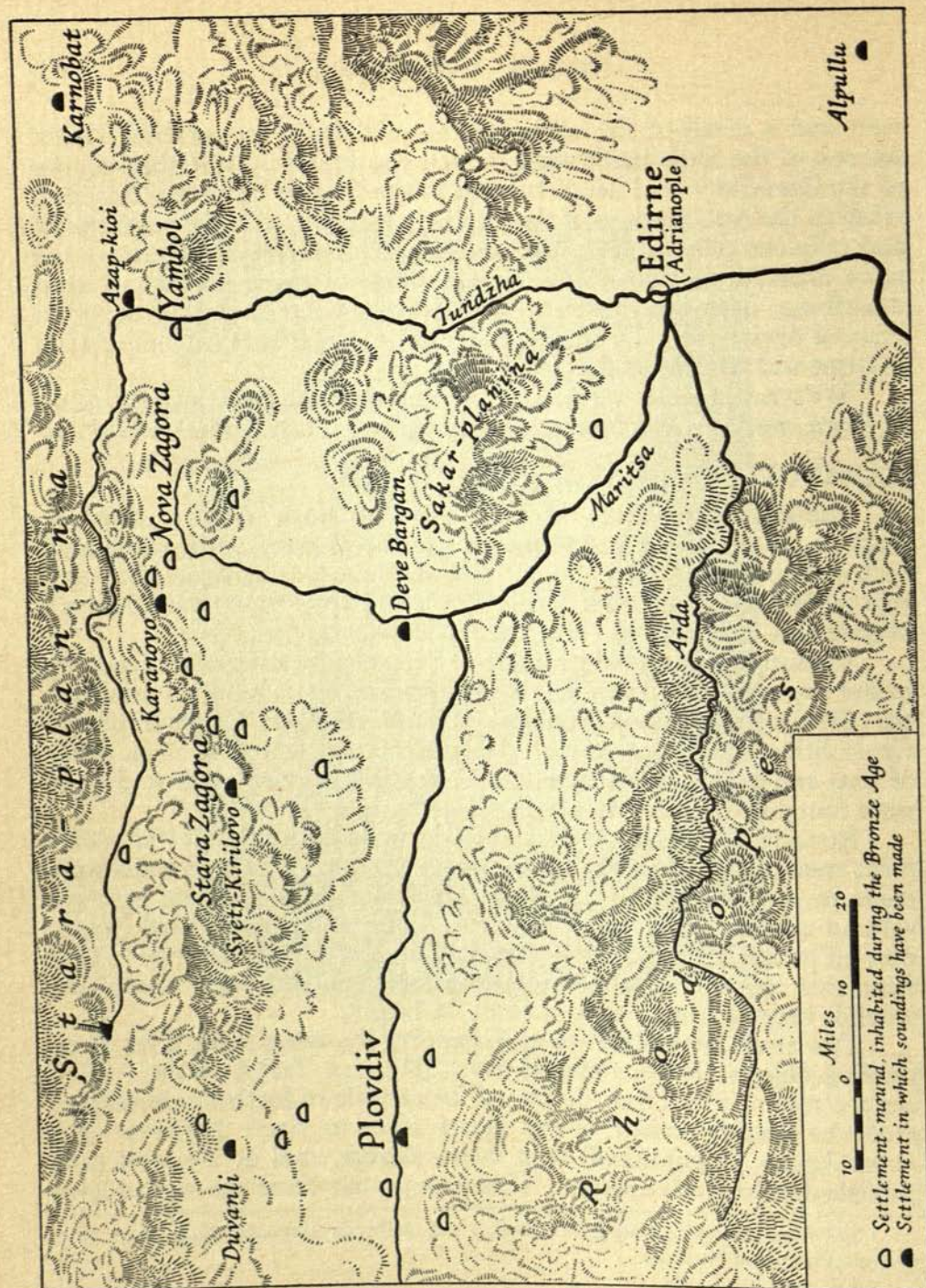
Recently the National Museum of Sofia conducted a trial excavation in the exposed eastern and southern slopes of this mound. It revealed the first definable stratigraphical superposition in Bulgaria of the Chalcolithic and Bronze Ages. Sherds, several complete pots, traces of huts and wall-plaster, stone and bone tools, animal bones and grain were found in both levels.

Remains of three rows of reinforced walls indicate that this settlement was a fortified village in its prehistoric past.¹ These walls were made of earth revetted with pitted limestone slabs set close together without mortar, and inclined inwards. The stone is of local origin, derived probably from Sredna Gora, 1½ km. north of Karanovo. Three fragments of a wall 1.10 m. wide were found on the south side of the mound, varying from 0.30 to 0.90 m. in height.

Two strata were observed at Karanovo, the lower level, 4 m. deep, yielding objects characteristic of the 'Bulgarian Tell Culture'. The upper stratum, whose 8 m. depth will be capable of further subdivision, produced pottery similar to the upper level at Sveti Kirilovo² and Asparukhovo (unpublished) near Stara Zagora, and at Zagortzi (unpublished) near Nova Zagora. This later pottery from the second level

¹ Tell Metchkur (B.C.H. 1906, p. 406 sq.) and Balbunar (*Izvestiia Bulg. Arch. Inst.* IV, 252-4) were also fortified.

² P.Z. 1914, pp. 64-75.



BRONZE AGE SETTLEMENTS, SOUTH BULGARIA

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is also similar to material found at Azap Kioi near Yambol,³ Shopova Mound (unpublished) near Stara Zagora, Graf Ignatief near Plovdiv (unpublished), and Duvanli,⁴ also near Plovdiv.

The chief differences between the two levels at Karanovo are found in the pottery. The remains of huts, millstones, flint and other stone artifacts, and the bone material, did not differ radically in the two strata.

In one section of the mound the remains of many huts were observed. They were quadrangular in form, and built on a pole frame, the walls made of clay mixed with straw, applied to wattling. The wall-plaster was found to have been fired to make it more firm. The huts usually contained a hearth in one corner, and a mill-stone in the other.

There was evidence that the huts had been destroyed by fire, and that, before subsequent rebuilding on the site, objects had been removed. Over the fallen wall-plaster of a ruined hut was laid new earth, on which the later huts were built.

Objects were found in only three of the huts. From one in the upper stratum on the south side of the mound came two complete vessels and the fragments of another. In the lower level on the east side the ruins of a hut were found to contain three vessels. In one of these pots three stone celts and a pile of mussel shells had been put, and in another a lump of white material for use in 'incrusting' decoration. A hut in the upper level on the east side yielded two vessels, sherds and clay sling-bolts.

The stone objects from Karanovo were varied, including mill-stones of typical Chalcolithic (Bulgarian) type,⁵ and celts, in size and shape like those from other Bulgarian mounds. A few fragments of perforated stone hammers and axes came from both levels; and a core from the perforation of a stone axe, was also found, indicating that the weapon was drilled on the spot. This core is doubly interesting, for the stone is not a local Bulgarian variety, but is probably derived from Anatolia. Several spheroid stone 'war-balls' were also found.

In the mounds of southern Bulgaria flint objects are considerably rarer than in those from north of the Balkan range, depending perhaps on the presence of natural flint outcrops, which are scarce in the south. But there are many in the north, in the Novi-Pazar, Shumen, Nikopol

³ Godishnik Plovdiv 1930, 171 ff.

⁴ B. Filov, Nadgrobni moghili pri Duvanli 1934, 26 ff.

⁵ V. Mikov, Stations et trouvailles préhistoriques en Bulgarie, p. 54.

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and Belogradchik regions. At Karanovo flint scrapers and knives, comparable to those from similar local mounds, were found.

A considerable quantity of bone and horn objects was found in both levels of Karanovo mound, including spear and arrow heads, the usual awls, chisels, and a perforated horn adze. These are like similar objects in previously published mounds. From the upper level at Karanovo came two bone pins of rather interesting type, with expanding, flattened, triangular heads.

But the most interesting object of horn was a sickle, found in the upper part of the lower cultural stratum. This curved horn, which is thicker at one end than at the other, is 31 cm. long, and 3.5 cm. thick at the handle end. Along the upper side, throughout almost its whole length, a deep groove had been made, in which were set a series of small jagged flints, four of which were found in position (3 to 4 cm. long, 1.1 to 1.4 cm. wide), set close together. The outer edges of the teeth were polished by use in cutting corn (PLATE III).

Similar flint-sickle teeth have been found in other Bulgarian mounds, and a few from Karanovo are deposited in the Nova Zagora Museum. Another, perforated at one end, was found at Malkata Peshtera⁶ near Tirnovo.

The pottery from the lower level (A) at Karanovo belongs with few exceptions to the Chalcolithic period. It is a handmade ware, the paste well cleaned. Its exterior colour is reddish, though it sometimes varies to dark black. The forms and decoration are characteristic Bulgarian Tell Culture types. In the upper part of the lower level were found high hollow bases, with and without perforations.

The decoration of this A ware was varied, including graphite painting and sherds ornamented with white painted lines, the lines easily being removed, and probably painted after firing. The graphite painted pottery occurred throughout in all depths of this level, linear decoration and wolf's teeth, etc., on red or on blackish ground, the patterns positive and negative (reserved). The white painted ware was found only in the upper part of the level, more frequently on vessels with high hollow base, and occurs only on red ware. The sherds illustrated are in soft white crumbly paint on polished red-brown ground.

Relief decoration (plastic ridges, etc.), finger-marked and incised designs were constant features throughout the levels.

⁶ R. Popov, 'Belyakovskoto Plato', p. 13.

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While the Chalcolithic pottery of Bulgaria is noted for its variety of ornament, the pottery of the subsequent period is almost without decoration. Most frequent were shallow incisions on the everted rims of the shallow bowls with four feet. Small zigzags, single or double lines, were incised on the walls and sometimes on the handles of a few of the vessels. Relief decoration was infrequent.

From all depths of the upper level came cylindrical handle fragments, the upper ends of which were furnished with a cylindrical projection, or wide and flat, or conic lug. All these handles were from large or small cylindrical-to-piriform pots (PLATE IV).

Bones of deer, gazelle, cattle, sheep, pig, etc., were found in both levels. In the upper level bones, teeth, and a hoof of the horse (*Equus caballus*). Until now no Chalcolithic settlement, either in Bulgaria or surrounding countries, has yielded horse-bones. But by the Middle Bronze Age the horse is in the group of domesticated animals. Some bones and teeth of horse were also found at Azap Kioi.

The prehistoric occupants of the Karanovo mound definitely practised agriculture. Wheat, vetch and varieties of peas were found in the lower level. More than ten bushels of carbonized wheat-ears, show that grain was stored for subsequent usage after harvest. In the upper level were some carbonized wheat and varieties of small peas.

The Karanovo mound is important in the prehistory of Bulgaria for the clearly observable presence of two cultural horizons, the lower four metres containing remains of the typical Bulgarian Tell Culture material. The deep upper level B, analogous in some respects to the upper part of Sveti Kirilovo, shows similarities with the Bereketaska Mound, with Zagortzi, Azap Kioi, Duvanli, and Shopova Mound, all of which are to be dated within the Bronze Age.

VASIL MIKOV,
National Museum, Sofia.

SUBMERGED BOAT AT NORTH FERRIBY (PLATES V-VI)

In the summer of 1937 we found between tide-marks on the Humber shore east of North Ferriby the projecting ends of three planks. A little rather destructive cleaning was done and the planks traced by prodding with a walking stick. They were found to extend for over forty feet at a depth of about three feet. It was now fairly obvious that the structure was the remains of a boat of some sort. Attempts were made to discover the method of construction and at first it was thought

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that the planks were pegged together. Later what seemed to be the bases of worn perforated cleats suggested that the boat was very early, and at one time we thought of a Viking date, but this was finally ruled out and we decided on further excavation. Three sections were uncovered, cleaned, photographed and filled in again. A large number of photographs were taken and careful measurements made. Early this year we removed about 6 feet of the eastern end, to experiment with methods of preservation.

It is now possible to give at least a preliminary description of the boat, which probably presents most of the essential details. We hope in time to remove the boat entirely, but the difficulties are great; the maximum working time between tides is about five hours, the boat is half a mile from any road and the sticky mud makes digging unpleasant. Probably the boat will have to be removed in short sections and may suffer a certain amount of damage in the process.

The boat lies obliquely to the line of the Humber, about 20 yards from low water mark and half a mile east of the end of the road at North Ferriby. It is buried in a stiff grey marl, and rests on a flinty gravel at the top of the boulder clay. At this spot the Ferriby Peat Bed and its underlying Green Seatearth had been eroded before the Grey Marl was laid down. The diagrammatic stratigraphical section of the site is shown in FIG. 1.

The boat is made of oak planks up to four inches thick. They are set edge to edge and the seams caulked with the moss *Neckera complanata* Huebr. Over the seams so caulked are thin battens of oak about half an inch thick and up to $2\frac{1}{2}$ inches wide. At intervals of about a foot are pairs of holes, one at each side, through which are threaded withes or ties of twisted yew, sewing the planks and batten together. On the inner plank at each join the tie does not go completely through the plank but comes out in the furrow at the side. (FIG. 2, PLATES V, VI).

There is a central dubbed out plank 43 feet long and from 20 to 24 inches wide. Two planks are preserved on each side, and from the yew-ties still in place at the edges of the outer planks it is clear that there has been at least one more plank on each side. At present the planks lie flat, but originally they must have looked in cross-section something like FIG. 3; and the western end of one plank still rises at an angle of thirty degrees or more. So far we have no evidence to show how the planks were supported in the curved position, but it was probably by cross-thwarts attached to the outermost plank.

So far as can be seen the centre plank is hewn from one trunk. The

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various projections on it are of a piece with the rest of the plank, and its western end is carved out. These projections are from east to west :

(1) Two groups of four rectangular knobs, 3 inches by 4 by 1, the two groups about a foot apart. Parallel to the western group there are two similar knobs on the next plank to the south.



FIG. 1. STRATIGRAPHICAL SECTION OF SITE OF BOAT
1, Grey Marl 2, Peat 3, Seatearth 4, Boulder clay
(Vertical scale exaggerated and diagrammatic)

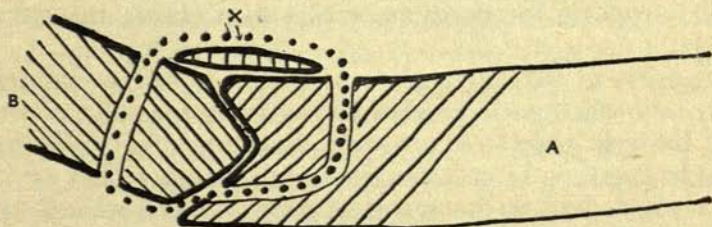


FIG. 2. METHOD OF JOINING THE PLANKS
Dotted line = Yew tie X = Batten A = Centre plank B = Side plank

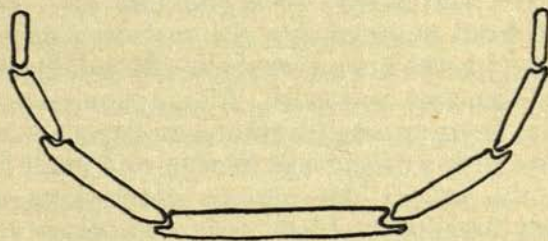


FIG. 3. PROBABLE ASPECT OF ORIGINAL CROSS-SECTION
OF EASTERN HALF OF THE BOAT

(2) To the west of (1) is a double ridge 8 inches long, 10 broad and 4 high lying in the centre of the plank. The area between the two parts of the ridge is not hollowed out right to the level of the plank.

(3) At the east end of section II (PLATE V) there is a double knob in the centre of the plank and in the middle of II a group of four irregular knobs, of which the two southern ones are L-shaped.

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(4) Just west of this the centre plank swells up to a height of four inches. Possibly the swelling is a block of wood repairing a hole, tied down with a cross batten which passes under the longitudinal ones (FIG. 5). The hole, if there is one, may have been due to a branch leaving the main trunk at this spot.

(5) In the middle of III there are two large projections, in line fore and aft, one about 4 inches square, one roughly rectangular, 18 by 4 inches. They both taper upwards and are about 5 inches high. (PLATE VI).

(6) Three feet from the west end of the boat there is a low ridge across the centre plank, like those on the floor of the Brigg dugout in the Hull Museum. That something was fixed to it is shown by a yew-tie still in position across its northern end. (PLATE VI).

The only suggested explanation for the various knobs is that they served to fix supports for decking or ribs to preserve the curvature of the sides.

Several joins in the battens were noticed. The overlapping ends are carefully smoothed and rounded off. At the east end of III (FIG. 6) one line of battens ends. Apparently the plank nearest the middle on the south side was not broad enough for the eastern half of its length and another plank had to be inserted here. The method of joining must be in doubt until further excavation. The same sort of thing probably happened just west of II (PLATE V) on the northern side.

Few objects were found in or near the boat. There were loose pieces of wood, oak and alder (*Alnus glutinosa* L.). Apparently reeds of some sort had been rammed into the surface of the planks. A few halves of acorns and hazel nuts, two shells of small crabs and a lump of white flint was all else that we found. About 50 yards away we recovered most of a paddle, from apparently the same layer of grey marl as the boat. It is of pine with a cylindrical handle, and blade flat one side and gently rounded the other. Its present dimensions are: length 40 inches, maximum breadth of blade 6 inches, diameter of handle $1\frac{3}{4}$ inches: the blade was originally about 7 inches wide.

Thus there are no external indications of date and we must consider stratigraphical and internal evidence. The former is not much use. The Green Seatearth below the Peat here is 'perhaps Maglemose'; some tufaceous marl and sandy peat from a buried stream bed in the peat is dated to before 1800 B.C. by A. S. Kennard on the evidence of about forty species of snails that it contains.¹ High up on the strand,

¹C. W. and E. V. Wright, 'Holocene Deposits at N. Ferriby', *Naturalist*, Sept. 1933, pp. 210-12.

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where the peat is only 6 inches thick, we found a flint implement *in situ*, according to M. Burkitt probably of Early Bronze Age. Thus any post-peat object at Ferriby is probably later than Early Bronze Age. The grey marl in which the boat is buried is merely an early Humber warp and an apparently continuous bed of it may well be of different dates at different spots. Stratigraphy only suggests that the boat is later than Early Bronze Age, which was fairly obvious already.

Internal evidence does not carry us much further. The boat is presumably of later type than dugout canoes and earlier in *type* than plank-built boats of Roman² or Veneti³ build. But it may be native built and Roman in date. It cannot be post-Viking, since the Humber keel of today suggests continuity with Viking vessels on the Humber and no post-Viking vessels had the planks sewn together. There is an attractive, but as far as we can see unsupported, conjecture that it belonged to the local Parisi, the tribe that inhabited east Yorkshire when the Romans advanced north. There was a settlement of these people on Red Cliff about a mile to the west of this site. A quantity of imported pottery of Tiberian and Claudian date, before the Roman advance to Yorkshire, has been found there by us, and it seems likely that there was for a time considerable trade with Gaul.⁴ However the attribution of the Ferriby boat to the Parisi is pure conjecture and at present we can hope for nothing better.

The closest parallels are the large 'sewn' boats of Ceylon and Oceania, and one suggested to us by Mr Hornell. In *Man*, 1928, 1, he described and figured some boats possibly derived from Indonesian culture, in use on Lake Victoria Nyanza in East Africa. They are made of large strakes on a broad dugout base, the seams are caulked with plantain fibre, covered with a strip of wood and sewn with twine passing through opposed holes in edges of the planks, as in the Ferriby boat. Except that the holes are close together and the twine continuous in the African boats the resemblance in these methods of construction is very close. Whether it is more than a coincidence we would not like to say.

We are indebted to Mr Hornell for the following note :

'From the absence of any trace of the presence of frames (ribs), I would conclude that this boat represents a crude and very early

² 'Ship of Roman period from site of New County Hall', L.C.C. 1912.

³ Caesar, *Bell. Gall.* III. 13.

⁴ P. Corder and T. D. Pryce, 'Belgic and other early pottery found at N. Ferriby, Yorks'. *Antiquaries Journal*, July 1938, XVIII, 262-77.

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development from the dugout canoe type having transverse strengthening ridges on the floor; it appears to be antecedent typologically to the Scandinavian prototypes of the Viking type of clinker-built boat in which frames are present, tied to lugs or cleats projecting from the inner surface of the planking. This view is strengthened when we compare the system of sewing the planks together in the Ferriby boat with that in the early pre-Viking clinker type—in the former by means of twisted yew ties passed through large holes spaced widely apart; in the latter with fibre twine sewing passed through smaller and more numerous holes.

The strangest feature of the craft is the unique manner in which the edges of the planks are fitted together and to the edges of the bottom dugout base-plank. In this system the lower edge of each side-plank is cut to a rounded bevel angle in order to fit securely into a v-shaped groove in the upper edge of the one to which it is joined. I know of no parallel to this, for the only craft in which the planks are not butted flat edge to edge (carvel build), or overlapped (clinker build) is the zigzag tongue and groove system seen in boats built on the Gujarati coast of India (see J. Hornell, *The Tongue and Groove Seam of the Gujarati boatbuilders*, *Mariners' Mirror*, pp. 310-12, 1930).

Our thanks are due to Messrs. M. W. Barley, H. H. Brindley, P. Corder, A. L. Congreve, J. Hornell, W. MacDowell, T. Sheppard and the authorities at Kew for suggestions and help in many ways.

C. W. and E. V. WRIGHT.

BYZANTINE WATER-MILL (PLATE VII)

The curious building illustrated is a detail of the mosaic which has been uncovered by the Walker Trust (St. Andrews University) excavations in the Great Palace of Byzantium. It is of some importance as being the only example of a water-mill in Roman art. Considering its late date—the mosaic is dated to the early fifth century—this is at first sight surprising, since we know that an ὕδραλέτης was among the possessions of Mithridates of Pontus which fell into Pompeius' hands in 65 B.C. (Strabo, XII 3.30). But in spite of the age of this, the first water-mill of which we know, references to water-mills are rare in Roman literature. Beyond the well-known epigram of Antipater (Anth. Pal. IX, 418, 3-6), a line of Lucretius (v. 516), and a remark of Pliny's 'Major pars Italiae nudo utitur pilo, rotis etiam, quas aqua verset, obiter et mola' (H.N. XVIII, 23), and excepting, of course, the

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description of Vitruvius' mill (x, 5, 2), the water-mill is not heard of until the late Roman period. This absence of literary record is paralleled by an almost total lack of evidence of actual mills. Except for the mill-wheel from Venafrum, in Naples, and the evidence of the mills by the Janiculum¹ and on the Wall² none are known.

From the fourth century onwards, however, evidence is more plentiful. Palladius (R.R. I, 41) recommends the use of the outflow of water from the public baths, a remark which implies a fairly wide distribution of mills, at least in the towns. Regulations for mills which must be visualized as being outside the towns are given in the Salic and Visigothic Laws,³ while Digest. xxxix, 2.24 may refer to mills as well as other buildings. Other references to water-mills are made by edicts of Honorius and Arcadius, A.D. 398 (C. Theod. xiv, 15.4), and of Zeno (undated; C. Just. xi, 43.10), and by Prudentius (contra Symm. II, 949-50)—all these referring to the mills by the Janiculum; and by Ausonius (Mosella 361-4), Venantius Fortunatus (Misc. III, 12, 37-8),⁴ and Procopius (B.G. I, 19). A fourth-fifth century epitaph of a *μανγαναρείος ὑδραλέτα* is recorded from Sardis.⁵

This literary evidence can fortunately be supplemented by the mill found in the American excavations in the Agora at Athens, and dated to the reign of Leo I (457-74).⁶ Enough of this remains to allow a reconstruction proving that it is based on the Vitruvian plan; the only innovation is that it is overshot. Vitruvius' mill was clearly undershot, as is the mill on this mosaic, and as were the only mills referred to in the sources about which the question can be settled—Ausonius' mill in the Moselle and Belisarius' floating mills in the Tiber. The overshot wheel is a development from the undershot, canalizing and increasing the force of the water. The literary sources show the extension of the water-mill in the later Empire; and until further evidence is obtained, the invention of the overshot wheel may be attributed to the later Empire too⁷—an argument in favour of more inventive ability than is generally allowed to have existed in that period.

¹ Van Buren and Stevens, *Memoirs of the American Academy in Rome*, I, p. 59 f and XI, p. 72.

² *Proc. Soc. Ant. Newcastle-upon-Tyne*, 3rd series, IV, 167.

³ Bennett and Elton, *History of Corn Milling*, II, 42, and *Lex Salica*, xxix.

⁴ For this and other references I am indebted to Mr C. E. Stevens.

⁵ Buckler and Robinson, *Sardis* VII, 138, no. 169.

⁶ A. W. Parsons, 'A Roman Water-mill in the Athenian Agora', *Hesperia* v, 70 ff.

⁷ As against Parsons' theory that Antipater was referring to an overshot wheel, see Bennett and Elton, *op. cit.*, II, chap. I.

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The curious and unfortunate fact about this picture lies in the inexplicable position of the wheel. If it is meant to be in the same plane as the wall on which it casts a shadow, the whole wheel should be visible; if in the same plane as the wall we do not see, it is in perspective from the wrong side. In either case, it is scarcely reconcilable with Vitruvius' description. The picture is an interesting and important corroboration of the evidence we already possess; but about the design of the mill it can tell us little.

G. BRETT.

THE PLOUGH AND TERRACING

In the article on 'The Plough and the Origin of Strip-Lynchets' by Dr E. Cecil Curwen in the March number of *ANTIQUITY*, there is a reference to the possibility of ground sloping at an angle of 20° being ploughed parallel to the contours without artificial terracing. There is on my farm here [Cressage, Shropshire], some pasture land which has been ploughed parallel to the contours, where the angle of the slope is in places 20° or even slightly steeper. This is almost certainly early 19th century work done with a swing plough.

A slope at an angle of 20° appears to be about the limit at which ground can be ploughed without terracing; it is much too steep to be ploughed uphill and it is probably equally difficult to plough it downhill. Most of the land in this county is now, and appears always to have been, ploughed at right angles to the contours, always excepting some very steep slopes (now pasture) which have been ploughed parallel with them.

True strip lynchets are very rare in Shropshire, I know of only one example and that a poor one. It is perhaps significant that this solitary example is situated on the Wenlock Limestone. W. J. SLACK.

RAS SHAMRA-UGARIT (PLATE VIII)

Monsieur C. F. A. Schaeffer very kindly sends us the following brief summary of the results achieved during the 10th and 11th campaigns in the winter seasons of 1938-39 at the site of Ras Shamra, the capital of the ancient kingdom of Ugarit in Northern Syria.¹

The Expedition discovered a new quarter of the town dating back to the middle of the second millennium B.C. The houses were large and each of them had a burial vault in the basement: both houses and

¹ *Résumé* of a lecture to the Académie des Inscriptions et Belles Lettres, Paris, 16 June 1939. We wish to thank Mr M. E. L. Mallowan for his help in this matter, especially for translating the note.

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tombs produced many objects, including clay and bronze vessels, weapons, seals, amulets and a variety of ornaments. Among the finds was a painted clay cup with walls of egg-shell thickness, a specimen of the celebrated Kamares ware, manufactured in Crete and imported to Ugarit. This discovery enables us to lower the date assigned to the end of Middle Minoan II by a century.

From a family vault of the 14th century B.C. we recovered the skeleton of an adult whose vertebral column had been pierced by a bronze arrow, which was still lodged in the centre of the medullary canal, inflicting a wound which according to Dr Jarry must have brought about almost instantaneous death. The arrow had entered the shoulder just as the man was about to attack an enemy who must either have been in a chariot, or else on some platform, at a level higher than that at which he himself was standing. This discovery was part of a chain of evidence proving the number of battles in which the city had been involved. Ras Shamra-Ugarit was not only a commercial and intellectual centre, but as the new discoveries now show, a perfectly equipped military stronghold, occupying a strategic position of great importance in Northern Syria.

In the official quarter of Ugarit the Expedition uncovered the royal stables, as well as the residency of the military governor, who was a son of the King of Beyrouth. Here we discovered a large clay tablet inscribed in the cuneiform character: this tablet is an inventory of the armaments kept in the arsenals and allotted to the soldiers who included bowmen and slingers. Near by, the excavations revealed another important building which was a part of the Royal Palace and contained the economic, diplomatic and private archives of the kings of Ugarit. These inscriptions sometimes make use of the celebrated alphabetic script invented by the scribes of Ugarit. They also make use of Babylonian, and sometimes the two different systems occur together. The new texts contain records of the taxes which the various cities in the kingdom had to pay and the number of soldiers they had to supply. There are several letters from foreign rulers, addressed to the king or the queen of Ugarit. One of these letters comes from the great Hittite king Shubiliuluma, a contemporary and enemy of the Pharaohs Amenophis III and IV, who were competing with him for predominance in this vital sphere of influence, a focal point on the trade routes of the ancient East.

Not far from the building containing the archives we discovered the Palace of Ugarit: the use of copper column bases overlaid with

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silver bear witness to its former richness. These bases recall the copper columns placed by Solomon in front of the sanctuary of his temple in Jerusalem and referred to in the Bible.

The Ugarit Palace was protected by an imposing fortified bulwark, the nucleus of which was uncovered in the course of the same campaign. It consists of a strong square tower, 14 metres in width, flanked by enormous glacis, which are entirely revetted by heavy stones. A vaulted subterranean corridor running from the interior of the fort to the foot of the glacis, and debouching on a formidable postern gate, enabled the defenders to rush the enemy in case of attack. This work is a revelation to us of the remarkable achievements of the military architects of Northern Syria as early as the middle of the second millennium B.C. Similar fortifications in Palestine, built on a more modest scale, may possibly have derived their inspiration from this source. (PLATE VIII).

Moreover at that time the port of Ugarit was one of the trading centres of Aegean-Mycenaean colonists in the East, and this work is therefore also an indication of the influence of Mycenaean military architecture, of which the principal monument is the fort uncovered by Schliemann at Hissarlik, generally reputed to be the Homeric Troy. Ugarit suffered the invasions of the Peoples of the Sea, who burnt and sacked it at the same time that they reduced Priam's city to ashes.

The Expedition also discovered a number of ritual foundations which contained large jars with perforated bases, deposited or buried vertically in the ground, intended to receive libations of water and other liquids. This ritual was designed both to secure a sufficient rainfall—this being essential to prosperity in a country with a scorching sun—and to refresh the dead who were buried in the vaults of the neighbourhood, sometimes in direct contact with these foundations.

The jars from Ras Shamra with holes in their bases explain² the meaning of the curious myth of the Danaids, which our own contemporaries still refer to without understanding its import, any more than the Greeks themselves understood it. According to the well known legend, the 50 daughters of Danaos who had been ordered by their father to kill their husbands on the night of their wedding feast, were condemned by the gods to pour water ceaselessly into a jar with no base to it. The significance of this very ancient rite had been forgotten by the Greeks and was interpreted by the mythographers as a

² See our *Cuneiform Texts of Ras Shamra-Ugarit*, British Academy, London, 1939, pp. 46-56.

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refined torture imposed by the gods upon these murderers. But actually, as the Ras Shamra jars with their pierced bases show, this was merely the ritual fulfilment of a funerary ceremony intended to secure eternal peace for the murdered husbands. If on the other hand we relate these jars to a fertility ritual, the result is just as helpful in supplying the key to the problem of the Danaids. For according to the oldest tradition of the story as related by Hesiod in the 8th century B.C., the Danaids were regarded as beneficent heroines who had brought water and fertility to the Argolid, a dried up country. In the earliest representations then, the Danaids appeared in the guise of water carriers filling one of these jars intended to produce fertility by the medium of appropriate libations, like those revealed by the discoveries at Ras Shamra. Indeed, on the earliest monuments representing the Danaids, the water running through the jar is not depicted by the artist as running out at its foot, for in accordance with the ritual it had to be dispersed deep down in the ground. This detail was only added under the influence of the legend on late monuments, at a time when the meaning of the task of the Danaids was no longer understood.

PHOTOGRAPHIC SURVEYS

Mr EDGAR T. BOND writes :—

‘ In connection with some historical notes on Porthpean, in Cornwall, I am just completing a photographic survey of that village. This consists of about 70 photos, taken on the chain system—*i.e.* each exposure within the viewpoint of the previous one ; it also includes several old ones taken about 20 years ago, and some from a photographic firm taken in the '80s. The latter show several cottages and paths now long disappeared.

‘ It soon became evident that to picture the village alone was not sufficient. The roads, paths and green lanes leading to it had to come into the plan wherever possible. Many may be altered or destroyed in the future and their present condition will then be of great interest.

‘ To make all this intelligible I am relating it to a large-scale map, about 12 inches=mile, showing the position and direction in which each picture was taken. Thus, no matter what the alterations, any particular place can be identified.

‘ All over the country there are members of archaeological societies who, with a little organization, could make similar records of these beauties of the countryside which are so rapidly disappearing. According to the Summer number of “Old Cornwall”, the Falmouth branch of

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the O.C. Socs. has already made a start. It is a chance to employ usefully a mass of dormant material that forms the "tail" of most such societies, and the more people you get to work the greater the interest.

'Any amount of such useful work could be done with an ordinary camera, but for a scheme such as I have detailed the cost would probably be prohibitive. With a miniature camera, however, it is quite moderate, and works out at about 50 exposures for £1—film, developing and "magnaprints" complete'.

THE CHAMBERED TOMB IN BEOWULF

In the great Old English epic of *Beowulf* the last exploit of the hero is, as is well known, a fight with a fire-dragon for the possession of a treasure which the latter guarded—a combat which resulted in the death of both. The poem gives a circumstantial account of the dragon's lair, which is described as beneath a great cairn (2213) *stanbeorh*, or a 'hoar stone' (2744)—*under harne stan*. Within the cairn was a cave (2410) or treasure house (2279) the work of giants, and with stone arches or vaults, literally 'bows', (2718) *stan bogan* resting on uprights. The dragon breathed out fire through an arch of stone (2545), and the outside of the lair is more than once referred to as a wall.

As Thurnam pointed out in 1869,¹ this is obviously the description of a megalithic chambered tomb. Stjerna, in his 'Archaeological Notes on Beowulf',² written c. 1903-5, makes no mention of Thurnam and seeks to identify the description with a confused recollection of Roman buildings, inserted by the English redactor of the poem. 'The Beowulfine Lays, which related to the fight at the grave-mound, were thus expanded and blended in England with other lays, in which a certain awe at the Roman remains, with their wonderful vault-construction, is exhibited'.

The point we wish to make, which has not apparently been noticed hitherto, is that while Stjerna's Roman origin is obviously untenable, there is evidence that the description is an alien interpolation into the Scandinavian legend. This evidence is the mention of vaulting or corbelling in the megalithic tomb. Now such construction is an early feature in Northern chambered tombs, having its prototypes in the corbelled *tholoi* of Palmella and Algarve, but is quite unknown in the late megalithic architecture of Scandinavia. It occurs however not

¹ J. Thurnam, *Archaeologia*, XLII, 202-4; quoted with comments by O. G. S. Crawford, *Long Barrows of the Cotswolds*, 29-30.

² K. Stjerna, *Essays on Beowulf*, trans. Clark Hall (Viking Club 1912), 37 ff.

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infrequently in the chambered tombs of Ireland and Scotland; New Grange and Maes Howe being outstanding examples. Both these tombs we know to have been broken into by treasure-seekers in early medieval times, for there is documentary evidence of the plundering of the Boyne tumuli in A.D. 861 or 862,³ while the walls of Maes Howe are covered with runic inscriptions recording its violation by Norse raiders.

We suggest therefore that in the description of the dragon's lair in *Beowulf* we have an account of a megalithic tomb, not Scandinavian, but more probably Irish or Scottish. Such a description might have been compiled by the original bard from the accounts of raiders returning from these parts, or it may have been added in England by one personally acquainted with such monuments.

ALEXANDER KEILLER and STUART PIGGOTT.

ROUTES IN SOUTHERN ARABIA

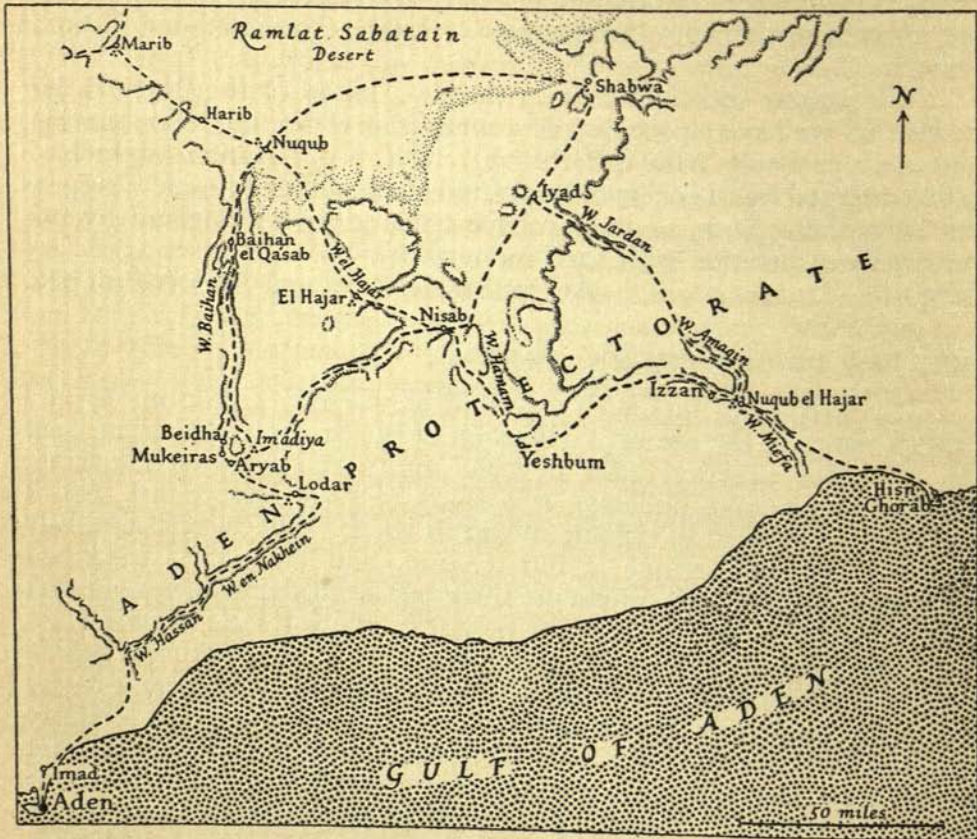
It is surprising that such a formidable corpus of inscriptions and proper names of the ancient civilization of South Arabia has been built up, while the practical basis is vague and often completely lacking. Semitic comparative philology is a study of great importance, but it must not be allowed to remain an end in itself. The most beautifully executed alabaster plaque is but a curio, and the names inscribed meaningless labels, if we cannot use them as historical documents, and get beyond the museum exhibit to the culture of which it is an expression.

An effort should be made to build up a working hypothesis, which would be of value even if in the end it proved wrong, provided it was based on the factor which has suffered least change—the country itself. If we take this view, it is more reasonable to make a start, not at the beginning with wild guesses and generalizations, but near the end of the long history where there is something concrete upon which we can get a grip. Once a small section of a road has been established, it is easy to travel either way. This figure of speech becomes almost literal if we agree that control of the fabulous Incense Route is the secret of the power and wealth of the early kingdoms. This indicates a study of communications, and if we can reconstruct a road-system from first-hand knowledge of the physical nature of the country, study of the inscriptions, and descriptions of the Classical geographers, we are in a position to take a first tentative step.

³ G. Coffey, *New Grange*, 25.

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The results of recent investigations in Wadi Baihan near the R.A.F. landing ground at Nuqub, and at 'Im'adiya indicate that the kingdom of Qâtabân flourished at periods in the early centuries of the Christian era. In his Geography,¹ Strabo writes that Kattabania lies



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to the south of the Sabeaen territory, and from the capital at Timna¹ controls the country as far as the coast. The site in Baihan was known to Glaser and Rhodokanakis as Kohlân,² and they were satisfied from the evidence in the inscriptions which were found that it was Timna¹,

¹ Strabo, 16. 4. 2.

² N. Rhodokanakis, 'Die Inschriften an der Mauer von Kohlân Timna'.

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although Mr S. Perowne came upon an inscription which gave the name of the town as Baihan, when he visited the site in 1938.

This is not the time to enter into an academic argument on the two names ; what we can say is that there is very little doubt that it was the capital of Qatabān, and it might be useful if we regard the area indicated by Strabo from a topographical angle. The great interest in the Nuqub site then lies in its strategic importance. If a military expert were handed a contour map with Aden, Marib, Shabwa, and Hisn Ghorab shown ; and asked where he would place a control point for communications, his choice would not differ much from that of the ancient builders. Situated at the bend where the Wadi Baihan begins to widen imperceptibly into the waste of the Ramlat Sabatain, it holds the fertile Wadi and the road south to Aden ; to the northwest Marib and the road to the north through Najran : to the east Shabwa and the road to the Hadhramūt ; to the southeast the road to Nisab and the coast. This is the basis of the accompanying map,³ which despite its elementary simplicity incorporates the latest available knowledge.

The route from Baihan to Aden is of interest, as 'Im'adiya fits perfectly into the general scheme. Although the site has not been properly surveyed since Mr Perowne first made it known, his description and photographs give a definite impression of a strongly fortified hill-post. It commands the road where it leaves the relatively open Soma Plain for the narrow pass to Lodar, and is the key position on the Baihan-Aden road. It is also possible to go from 'Im'adiya to Nisab, but unfortunately little is known of this part. Between Lodar and Aden there is nothing of any importance known, except at Imad, about fifteen miles from Aden. On the ground it is a great sand-dune but from the air it assumes a definite shape, and the declination of the sand seems to indicate a straight road to Aden.

The names 'Im'adiya and Imad have a similar ring but it would be very unwise to deduce a connexion on that account. Although the traditional memory of the tribes is remarkable, there is very little to be learned from them concerning the ruins. Few of the sites have a significant local name ; hence the recurring 'El Hajar', 'the Stones' which is evidence of the complete lack of interest. On making enquiries about a stele in Baihan, the Sharif assured me that it was very old as it had been there in his great-grandfather's time !

³ The map is on the International Scale, and wherever possible the readings of the standard maps have been checked by R.A.F. Survey readings, and in many cases corrected.

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In Aden itself, there is no trace or justification of the flattering 'Arabia Eudaemon'⁴ save in the famous 'Tanks'. Although the wonderful cemented rock-cisterns in Taweela Valley have been repaired and reconstructed beyond redemption by the British Authorities, a careful walk round Crater reveals traces of nearly fifty such interconnected cisterns—the remains of a gigantic water conservation system. The builders and the reason for their work, as well as the period of construction, are problems to which no one has attempted to give serious answers. Indeed it is questionable if the extent of the water-system has been fully appreciated. There are reports of cisterns and dams from all parts of the Aden Protectorate and the Yemen, and it is interesting to note how they conform to a set type, as though the principles of construction were the same for all. A comparative study would not only be interesting in itself, but might enable us to get an idea of the difficulties which the builders were facing. Then apart from the water question it is worth noting that even to an inexperienced eye there is a striking resemblance in the finish of the cement work in the untouched cisterns in Aden, and the ruins in Baihan.

Although the Baihan-Aden road is an attractive prospect, it is nothing when compared to the great highway of the ancient world, from the south coast of Arabia to Petra in the North. This was the vein of gold which made Arabia Felix. Before we attempt to find the Incense Route, it is useful to lay down the major qualifications which we regard as essential. It should be the shortest practical distance; in a desert area, one with assured water and food supply; and from the all important aspect of security, one which can be protected.

If we attempt to follow the route in the Aden Protectorate, we must have two definite points to work from, which are Hisn Ghorab, the site of Cana, the sea-port where the road began, and Marib, beyond the Protectorate frontier. This is not assuming too much, as Hisn Ghorab has been recognized as the site of Cana by every reputable authority from Glaser onwards,⁵ and the locality and importance of Marib is past doubt.

There is only one route which fulfils the necessary qualifications, and that is a road from Hisn Ghorab *via* Nuqub el Hajar, Nisab, El Hajar, Nuqub ('Timna' or Baihan), to Marib. The first two depend on the natural features of the country and are still valid, but the security of the road is more difficult to establish. What we can say is that it was capable of defence and there are indications that it was actually defended. Hisn Ghorab is a fine natural harbour with extensive ruins

⁴ Periplus, 26.

⁵ Glaser, 'Skizze der Geschichte und Geographie Arabiens', 174.

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which are a rough indication of its ancient role as the frankincense port. The first ruins of any importance are at Nuqub el Hajar, and a glance at the map reveals their significance. This strongly fortified position, with its great water cisterns of the Aden type, is situated at the point where the road leaves the flat coastal plan for the narrow wadis. The next known fortified position is at El Hajar, between Nisab and Nuqub, where the wadi narrows and is easily defensible.

There is very little accurate information available on the road between Nuqub el Hajar and Baihan, but many conversations with Political Officers, R.A.F. Officers, and tribesmen from this area, confirm that the route is open, well watered, and still in general use. There are many stories among the Arabs of the Nisab section of 'written stones', but as yet no investigations have been made.

In view of the fact that Shabwa has been identified with the Sabbathath of the ancients, the great centre of the incense trade, some explanation is due for its exclusion from the route suggested above. Shabwa was a magic name which could be transformed from Sabbathath to Sheba, and the legend was never seriously challenged until in recent years it has been visited and surveyed. The great city which Pliny pictures, with the sixty stately temples within the walls,⁶ was not what Philby found, and there is no evidence of past greatness on this scale. Although it would be foolish to ignore the works of the Greek and Roman writers, from Eratosthenes to Pliny, it is equally so to regard their every statement as sacrosanct, and distort modern discoveries to fit their framework. There are no indications near Shabwa of any great road leading south to Hisn Ghorab. The shortest route would be through Iyad, and to Nuqub el Hajar *via* the Wadi Amaqin, but it is doubtful. Even if the road did go through Shabwa, there is no road to Marib except through the waterless Ramlat Sabatain, which is out of the question as a trade route, and in fact the only road from Shabwa is *via* the ruins in Baihan. Moreover Shabwa was a Hudramūt stronghold, and in the majority of the inscriptions in which this state is mentioned with Qatabān, they are at war or on unfriendly terms.

Unless the route which has been outlined is completely wrong, a proper survey should reveal a great natural highway linked by traces of its ancient glory. In view of the great work which has been done in the Euphrates Valley and Palestine, it is to be regretted that the civilization of Arabia Felix should be left shrouded in sand, and we remain little better informed than the Greeks two thousand years ago. JAMES DUNCAN.

⁶ Pliny, 6. 32.

Reviews

ASINE: RESULTS OF THE SWEDISH EXCAVATIONS 1922-1930.

By OTTO FRÖDIN and AXEL W. PERSSON. *Stockholm (Generalstabens Litografiska Anstalt)*, 1938. pp. 452. 52 KR.

English readers have every reason to be grateful to our Swedish colleagues for publishing this book in English. We trust that they will show their appreciation of this courtesy by purchasing the work. They will certainly get their money's worth. The five campaigns directed by Dr Frödin produced a wealth of material invested with the highest scientific value owing to the accuracy of observations on the conditions under which finds were made. As interpreted by Prof. Persson it throws a great deal of new light on the earliest civilizations of Greece and on their relations with Crete, with Anatolia and with the north.

In the Early Bronze Age Asine was already an important port. Bales of merchandize, sealed in Crete, were landed there in Early Helladic times. These sealings provide the best direct evidence for dating the E.H. period in terms of the Minoan system and so of our own reckoning. The stratification of the successive settlements provides additional evidence for Mainland culture-sequence. The latest E.H. floor deposits are sealed by a burnt layer, as at many other sites. Within the E.H. period itself the ceramic remains at least can confidently be distributed over three phases, the stratigraphical sequence of which is objectively illustrated by the sherd-count given on p. 201. The Minoan sealings and a stone seal, remarkably like a vith Dynasty Egyptian specimen, are attributed to E.H.III (the possibility that the seal might belong to an earlier phase does not seem to be quite excluded by the observations actually recorded in the text).

The succeeding M.H. age is likewise subdivided into three phases whereas most American and English excavators have hitherto only been able to distinguish two. Persson recognizes an admittedly brief M.H.I. phase distinguished by the presence of hand-made Minyan ware, but a preference for shiny surfaces and the absence of matt-painted ware. The latter fabric first appears in his M.H.II which should therefore correspond to the M.H.I of Blegen and Wace. We are, however, like Persson himself, 'struck by the way in which the first eras of the Late Helladic period, L.H.I and L.H.II are presented by their characteristic pottery. Occupation layers proper, nowhere appear, and wherever they occur it is in connexion with sherds with simple, linear decoration of the kind looked upon as typical of L.H.III'. If there be in fact no L.H.I-II occupation-layers, does it not follow that M.H.III covers also L.H.I and L.H.II, i.e., that

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Minyan and matt-painted wares remained the normal fabrics current at Asine (as apparently at Eutresis too) down to L.H.III times while at other centres L.M.I-II vases were being imitated in the classical L.H.I-II pottery? If so, the Asine succession alone could hardly be used in evidence for raising the beginning of the Middle Helladic period to 2000 B.C.

Within the chronological framework thus established, a number of new observations are recorded. As samples we can only mention the use of tiles for roofing in E.H.III, the presence of sherds of distinctly Macedonian pottery in the M.H.II-III horizon and of a new group of coarse wares which reappear in sub-Geometric graves. Persson's familiarity with Turkish museums has enabled him to recognize a close parallel to an E.H.I pot in the Copper Age of Alisar, a probable Early Hittite import in M.H.III and the (indirect) connexion between his M.H.II matt-painted ware and the Early Bronze Age pottery of Cappadocia.

In his very interesting conclusions Persson agrees with most other authorities on the Anatolian ancestry of Early Helladic culture. But the technique of glaze painting, attested first in E.H.II, would be derived from Crete. The intrusive culture of the Middle Helladic period would be due to the first Indo-European Greeks and derived from north of the Balkans. A parallel wave of invaders from the same quarter would have crossed the Dardanelles, whence some spread southwards coastwise while others advanced to Central Anatolia to form the Indo-European ruling class in the Hittite Empire. A reunion of the two streams would be reflected in the Anatolian traits recognizable in M.H.II-III (matt-painted ware, jar-burial for children). As northern traits in the M.H.I culture our author claims contracted burial, perforated axes, antler sleeves and picks, and clay seals. But to call contracted burial 'a Nordic feature' (p. 349), is absurd. It suffices to mention Predynastic Egypt to invalidate the inference. Perforated axes seem older at Troy and Thermi than beyond the Balkans and at Malthi in Messenia are assigned by Valmin to E.H.III. The clay seal is not particularly like the Danubian and Thracian 'pintaderas' which in any case are usually regarded as copies of Asiatic or Minoan stone seals.

Nevertheless Persson's thesis has much to recommend it. His observation that, in M.H.I and M.H.II, Minyan ware was made without the wheel does away with one of Forsdyke's arguments for bringing this pottery and its makers from Troy. Parallel inroads into Greece and the Troad would seem the proper solution. In any case the evidence from Asine is here set out objectively. The book contains many other facts and deductions which cannot even be mentioned here. The text is handsomely illustrated by three coloured plates, six folding plans and 275 text-figures. It thus forms a model publication worthy of a place beside the great American and English monographs that have been devoted to other Mainland sites.

V. GORDON CHILDE.

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ZAKLÁDY HALLSTATTSKÉ PERIODY V ČECHÁCH (The Dawn of the Hallstatt Period in Bohemia). By JAROSLAV BÖHM. *Praha*, 1937. pp. 299.

The hill-girt basin of the Upper Elbe, interposed between the Danube valley and the great plain of northern Europe with its copper and tin and fertile lightly-wooded löss, was no less important in prehistoric than in recent times. It has always been a junction where natural routes converge, a coveted prize for barbaric neighbours. The relics that crowd the local museums faithfully reflect the contacts with other regions established by the trade and industry of its population or by brutal aggression from outside. Monographic studies of this material by Czech prehistorians, unbiased by any 'Nordic mythology', have therefore always been at the same time authoritative contributions to general European prehistory. While the political boundaries of Czechoslovakia still corresponded to those fixed by nature, Dr Böhm was able to compile such a monograph on the dawn of the Hallstatt period that is a worthy successor to those of Stocký, Schráníl and Eisner on the neolithic, and Early and Middle Bronze Ages. His conclusions, based on a thorough analysis of all the collections (largely unpublished) are of first-class importance for such issues as the absolute chronology of the Bronze Age and the origins of the Kelts. Not only the Knovíz pottery here described, but also the shepherd's crook pins associated with it, have a direct bearing on British archaeology.

Bohemian prehistory is here depicted in a complexity which comes as near historical reality as archaeology is likely to get. The country is subdivided into four regions, the parallel developments in, and interactions between, which have all to be considered. When the study opens about 1400 B.C. (Reinecke A2) the Aunjetitz culture was firmly established in central as in northern and eastern Bohemia, but the tumulus-culture was already emerging along the Danube-Elbe watershed to the southwest. The connexions of the latter lie south of the Danube in the Sudeten-Danubian province (even the flutings on advanced tumulus pottery can be derived from the southeast whence it would independently have come to Lausitz and southwest German ceramics). Five stages of development can be distinguished. By the third stage ('Kbely-Řepeč') the tumulus-culture was spreading into central Bohemia to be superimposed upon the Aunjetitz culture. Remnants of the Aunjetitz population doubtless survived, but they made no recognizable contribution to the local culture of the period (Reinecke C 1225-1100 B.C.) which is completely assimilated to that of the tumuli.

After 1100 (Reinecke D) the development of the mixed culture in central Bohemia is disturbed by the intrusion of the Lausitz culture. The latter originated, according to Böhm, north of the Riesengebirge between the Elbe and the

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Warthe with an actual infiltration from the Sudeten-Danubian province through the Moravian Gates to impinge upon the Silesian Aunjetitz culture. The Lausitz culture emerges in northern Bohemia even before 1100 B.C. (in Reinecke c). Then, superimposed on the central Bohemian tumulus-culture with its suppressed Aunjetitz survivals, it produces a new hybrid, the Knoviz culture.

In central Bohemia the latter develops through four phases to culminate about 700 B.C. in the explicitly Iron Age Bylany culture. But the Knoviz, like the Lausitz, culture exhibits a strong tendency to expansion. Spreading south-west, in the wake of an earlier and rather vague Lausitz stream, it overlaid the local tumulus-culture, transforming it into the Milaveč culture. The Milaveč culture in southwestern Bohemia reaches the zenith of its development in Hallstatt A between 1000 and 850 B.C. and thereafter seems to vanish. But beyond the divide, the expanding Knoviz culture, superimposed upon local branches of the tumulus-culture, can be traced as a principal component in the southwest German and Tyrolese urnfields. An independent expansion of almost pure Lausitz culture is, however, admitted as responsible for the urnfields in the Main valley and the Middle and Lower Rhine.

By a revision of connexions with Italy and the Aegean Böhm justifies a short chronology. The later phase of the Aunjetitz culture (Reinecke's A2) begins about 1400 B.C., a date which could now be justified by Beck's and Stone's dates for segmented faience beads, but must last till nearly 1200. Sub-Mycenaean parallels to the violin-bow safety-pins from Lower Austrian and Tyrolese urnfields put the last phase of the pure Bronze Age (earlier Knoviz) between 1100 and 1000 B.C. Spectacle-brooches such as first appear in Hallstatt A (later Knoviz) are dated in Thessaly by Heurtley between 1000 and 800 B.C., while the antennae swords and crescentic razors of the succeeding period are assigned in Italy to 850-700 on the chronologies of Åberg and Ducati. The beginning of the succeeding Hallstatt C-D Bylany culture about 700 B.C. is deduced from Arnoaldi imports, while its end by 400 B.C. follows from Jacobsthal's dating of the succeeding La Tène phase. In an interpretative section at the end the author asserts that the battle-axe folk, in a wide sense, are to be identified with Indo-Europeans and argues that central, southern and western Bohemia should be regarded as forming parts, albeit peripheral parts, of the Keltic cradle.

The Czech text is accompanied by a German resumé of 33 pages but a fuller German work treating of the period throughout the Republic is promised. Now it would seem eminently desirable that the present book, perhaps the last account of a controversial period that can appear in Central Europe unaffected by politically inspired dogmas, should be translated *in extenso* into English. In that case some modification of the terminology for the benefit of non-Slavonic readers

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might be advisable. Here each phase in the evolution of four cultural series is normally denoted by a pair of Czech place-names, like Drhovice-Vrhavěč. A foreigner may be expected to learn the names Knovíz, Milaveč, Bylany, as he has Aunjetitz and Lausitz. The phases of the cultures thus designated could surely be indicated by the addition of letters or numbers. The maps might also be printed on a rather larger scale.

V. GORDON CHILDE.

THE POTTERY OF SANTO DOMINGO PUEBLO : a detailed study of its decoration. By KENNETH M. CHAPMAN. *Memoirs of the Laboratory of Anthropology*, vol. I. *Santa Fé, New Mexico*, 1936 [received Dec. 1938]. pp. xiv, 192, 80 plates, 34 text-figures. Price \$4.00.

A detailed study of this interesting pottery, which the author has been collecting for nearly twenty years. Mr Chapman lays particular emphasis upon the fact that Santo Domingo decorative art has not been influenced by contact with Europeans. 'Pueblo pottery of the past three centuries', he says, 'proves to be a normal development, in which scarcely a trace of European contact can be detected'.

The author's description of the decoration he has so carefully studied perhaps suffers from too much analysis. I mean that it considers perhaps in too abstract a way the decorative elements as single units, first isolated, then arranged and combined by the potter's skill. Nevertheless, it seems quite conclusive as to the stability of the traditional Pueblo art; it shows, too, that a certain margin of adaptability to new types of life and new techniques may give promise of a survival of indigenous pottery-making. In spite of the well-known Pueblo conservatism, some innovations have appeared. Mr Chapman points to the real problem which is now in view, that is, the necessity of giving this craft a sound economic basis, the lack of which accounts for the decline of Zuñi pottery. Interesting efforts are now made in the southwest United States to resolve this problem.

Mr Chapman also gives much useful information on the methods used for pottery-making and painting, and upon the symbolic meaning of some decorative elements.

JACQUES SOUSTELLE.

THE 'RAIN BIRD' : a study in Pueblo design. By H. P. MERA. *Memoirs of the Laboratory of Anthropology*, vol. II. *Santa Fé, New Mexico*, 1937 [received Dec. 1938]. pp. 113, 48 plates. Price not stated.

This is a very careful study not only of the so-called 'Rain-Bird' motive, but of the evolution of Pueblo pottery from the standpoint of decoration. The writer shows how the elaborate decoration of the historic forms gradually evolves from the rudimentary stages of the prehistoric times. Generally speaking, it

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seems that pottery appeared in the southwest of what is now the United States during the early centuries of our era (' Basketmaker III period ' as it is technically known). At this stage, decoration consisted only of lines and dots. Mr Mera traces the evolution from those crude motives to the refined decorative system used by the Zuni, Hopi and other Pueblo potters.

The ' Rain-Bird ' motive is one of the most generally used by the Pueblo, as it is found practically in all their towns. The author is quite right, I think, in emphasizing the effective and traditional value of the decorative art in a tribe where the greatest conservatism still prevails. The plates are quite good.

JACQUES SOUSTELLE.

DER WEG AUS DEM CHAOS. By PAUL LIGATI. *Callwey, Munich, 1931* [received 1938]. pp. 296, 317 figs.

The idea of a cycle of repetition in civilizations was known 2000 years ago to the Etruscans (Plutarch, Sulla). The study of early civilizations in Egypt had, about 30 years ago, led me to a similar result, explained in *Revolutions of Civilisation*. At that time the glamour of universal progress, started in the Exhibition of 1851, made any idea of a reversal heretical. We know better now in Europe. *The Downfall of the West* by Spengler was startling; it is now a commonplace.

The most detailed exposition is by Paul Ligati of Buda-Pesth. In 296 pages of close text there are, however, no references given to the 317 illustrations which follow. The figures are so much diversified, from Modern to Ancient Egyptian, that it seems impossible to recognize a clue to their purpose, in the absence of any references to the text.

The many diagrams provide a wavy outline of civilization, with names in every part. In music, Handel and Wagner are on a rise, and Mozart and Beethoven in a fall, the application of which is obscure. The Venus of Milo, by the way, is entered a couple of centuries too late. As in different diagrams the length of periods varies from 70 to 150 years, no general system in civilization can be demonstrated.

The conclusion is that it is the mission of Genius to receive from the heights and bring down to mankind—a task, a moral duty, towards the remainder of mankind.

FLINDERS PETRIE.

THE CAMBRIDGE REGION. Edited by H. C. DARBY. *Cambridge University Press, 1938.* pp. xiv, 234; 58 text-figs. 6s.

The meetings of the British Association at Cambridge in 1904 and 1938 were both marked by the issue of a scientific survey of the Cambridge district. The earlier volume, the well known ' Handbook to the natural history of

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Cambridgeshire', edited by Marr and Shipley, described the geology, zoology, botany and prehistoric archaeology of the county, while in the later volume here under review thirty-four authors have covered the same field and included much else besides. A comparison of these two books affords an interesting commentary on the progress of research in the last generation.

The chapter on geology and physiography is based, like other sections, on material prepared for the recently published first volume of the *Victoria County History of Cambridgeshire*. Here the most notable additions to knowledge concern the underground structure of the district, especially the nature and depth of the Palaeozoic floor now being studied by seismic methods, a line of research unknown in 1904, and the post-glacial deposits of the Fenland where detailed exploration has begun with the combined efforts of archaeologists, botanists and geologists. Accounts of the soils and climate, not included in the 1904 handbook, are valuable contributions in a district so largely agricultural. The main types of top soils are clays from boulder clay or Jurassic and Cretaceous rocks, chalk on the northeast-southwest extent of downland, and fen silts and peats in the north half of the county. This distribution controls not only the types of farming, noted in the chapter on agriculture, but also village-sites (compare text-figs. 24-25 with text-fig. 4). The heavy clay soils are the least productive; on the chalk belt farms are large and sheep the livestock traditionally associated with this district, while in the Fenland production is much more intensive, especially on the extremely fertile alluvial silts. Half of the botanical chapter is devoted to the Fenland, in many ways the most remarkable area in the Cambridge region. Here there is a great advance on the 1904 account, which consisted mainly of lists of plant-associations. The increase in knowledge in the last 30 years now permits an ecological approach. The natural vegetation successions can best be studied in those communities not seriously altered by agriculture, and Cambridgeshire is fortunate in including more of the peat fens than any other county and in particular Wicken fen, perhaps the largest area of uncultivated fenland. The zoological section omits the long lists of species in the corresponding part of the *Victoria County History*, and in so doing gains for the general reader who seeks an up to date summary of the fauna of the county.

It is in the archaeological chapter that the contrast with the 1904 handbook is most marked. A generation ago regional distribution studies had not begun, and the account of the prehistoric archaeology of Cambridgeshire described the trackways, the defensive dykes and finds of small objects. The work of the subsequent thirty years, which includes the distribution studies of Sir Cyril Fox, the investigations of the Fenland Research Committee on the post-glacial sequence in the fens, and of the Cambridge Antiquarian Society on the great

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dykes and Anglo-Saxon cemeteries, has far more than local importance. The relation between the areas of human habitation and land movement is well illustrated by the distribution diagrams, which show a retreat from the Fenland between the Bronze Age and Early Iron Age. In the Roman period there was a re-advance into this district, where much of the silt area in the north of the county was intensively cultivated. This is almost the first account to be published of the great Roman agricultural area and its recognition completely alters the picture of Roman economy in this part of the country. The extent of the visible earthworks of these Roman villages and the ease with which they can be traced by air-photography is not generally realized.

The concluding chapters are devoted to Cambridgeshire in the nineteenth century, the growth of Cambridge and a description of the Breckland. The account of the draining of the fens contains much information about a subject prominent in recent years. Extension of the drainage with consequent shrinkage of the peat and progressive lowering of the surface seems likely to cause the Ouse Catchment Board more trouble in the future, unless the outfalls can be improved by promoting natural scouring of the river mouths.

This book is a fine example of a regional study produced by a team of writers. It provides a most useful summary of the Cambridge district, and the University Press is to be congratulated on its re-issue, which makes it available to a wider circle than members of the British Association. It is to be regretted that the geological map, also specially prepared for the Association's meeting, is not republished with the volume, which includes no large scale map of the district that it describes.

K. ST. JOSEPH.

LA PRÉHISTOIRE. By A. VAYSON DE PRADENNE. *Paris : Librairie Armand Colin*, 1938. pp. 224, 47 figures. Price ascertained to be 15 francs.

This book is volume 210 of the 'Collection Armand Colin', a series of popular manuals which aims at giving clear and precise expositions of the present state of knowledge in a very wide range of subjects. In the present volume world prehistory down to the close of the Neolithic is dealt with in 220 pages, of which 72 are devoted to method and technology, so the treatment is necessarily very summary indeed. The book is divided into three sections. The first, which is introductory, deals with definitions and programme, method, the search for prehistoric remains, and technology, and is probably the most valuable part of the work. Considering the limitations of space imposed on the author it contains a remarkable amount of information, presented with great clarity and economy of words. The chapter on Technology obviously owes much to M. Vayson's own technical training; the descriptions of the process of working flint and other stones, and of the different types of tools, are much

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more intelligible than is usually the case in text-books of this kind. The second section, Classification and Chronology, which deals with Western Europe, is also very clear, but suffers more from unavoidable compression. The chronology of the Lower Palaeolithic, for example, is a highly complicated problem, and although M. Vayson states very lucidly his personal views, it is hardly possible for him to do justice to those of other schools, and in particular of the Abbé Breuil and the group which works in collaboration with him, and he does not seriously attempt to do so. The last section, which deals with discoveries outside Europe, though again very much compressed, is admirably up to date; this is probably the only small manual which attempts to include the latest discoveries all over the world.

Within the limits imposed by its size, and by certain predilections of its author, this book should provide a useful introduction to prehistory for the public to whom it is addressed.

D.A.E.G.

LE PALMIER-DATTIER ET LES ARBRES SACRÉS DANS L'ICONOGRAPHIE DE L'ASIE OCCIDENTALE ANCIENNE, *par* HÉLÈNE DANTHINE. *Text*, pp. 279, *album of plates with 1217 figures*. Paris: Paul Geuthner, 1937 (received November, 1938). 300 Francs.

This admirable work is produced in a manner worthy of the importance of the subject treated. The excellent types selected for the text and the well-spaced pages make reading a pleasure, and the 1217 illustrations, mostly in line-drawing, are clear and adequate for the purpose. They are, indeed, so numerous and cover such a wide range of ancient art that it would be difficult to find a representation of a sacred tree which is not included. Scholars will feel an immense debt of gratitude to Dr Hélène Danthine who, by her thorough and penetrating analysis of the available material, has spared them the trouble of further search for examples.

Very few ancient texts treat of the sacred tree apart from a text descriptive of a New Year ceremony, the legend of the dark *kiškanu*-tree which grew in Eridu, and a few scattered allusions. But in this book the brilliant analysis of all the evidence shows how much can be learnt even from such meagre sources.

The title of the book gives an accurate indication of its contents. The tree considered in its religious significance was 'sacred', but it was not a 'tree of life' in the sense that to eat of its fruit could confer immortality. The palm-tree is rightly named first in the title, for it was the sacred tree *par excellence*. For this reason the authoress begins with a valuable botanical study of the palm-tree, its area of diffusion, its characteristics, and the methods employed in its cultivation. This careful investigation compels her to reject both the widely-held theory that scenes in which genii hold a cone and a pail represent an actual

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fertilization of the sacred tree near which they stand, and the alternative theory which would explain the scenes as a ritual aspersion. She concludes that any idea of realism is excluded by the artificial character of the tree and the aspect of the genii, and believes that the scenes can only be symbolic representations of artificial fecundation.

A detailed examination of the forms, sometimes exceedingly abstract, which the sacred tree assumed in the course of centuries is followed by a discussion of the scenes in which the tree figured. One of the problems of the subject is to decide whether any given delineation of vegetation depicts a sacred tree, or is merely a tree as an indication of landscape. It is true that some scenes, which seem to give no indication as to whether they have or have not a religious meaning, are proved by comparison with more complete or precise specimens to fall into the category of sacred trees; in this book every tree has been given the benefit of the doubt to avoid the omission of an example which might by a remote possibility be classed as sacred. Professor Walter Andrae has pointed out (OLZ 1938, s.508f.) that three phases of the sacred tree are represented, namely, with bare twigs, with leaves and flowers, and, lastly, laden with fruit, thus corresponding to the three functions of life.

The authoress describes plants of diverse species which were sometimes pictured apparently in the place of the date-palm. But she offers no explanation as to why the conifer was so largely represented during the Akkadian period, and to a lesser extent in the Larsa period, and whether it had exactly the same significance as the date-palm. It seems probable that the date-palm and the conifer both implied 'fertility', but that, as the fundamental idea which they symbolized was not identical, both of them signified in addition particular attributes derived from the sources from which they originated.

A description of the compositions in which the tree figured leads inevitably to an attempt to interpret their meaning. Mlle Danthine ably summarizes the views held by various authorities on the subject, and this gives her an opportunity to consider the connexion between the sacred tree and the two trees of Paradise, the tree of life and the tree of truth (E. Dhorme, *L'arbre de vérité et l'arbre de vie: Revue Biblique* (1907) IV, 271-4; A. Ungnad, *Die Paradiesbäume: Zeitschr. der Deutschen Morgenländischen Gesellschaft* (1925) LXXIX, ss. 111-118). In this respect it might have been explained that the reason why a single tree or, still better, two trees are frequently introduced into scenes where two divine attendants open the wings of a door or hold a gate-post is because they typify the two trees which stood at the door of Paradise, the *giš-ti* and the *giš-zi-da*, guarded by the two guardians of the gate of Anu's palace, Dumuzi and Ningizzida.

Upon a subject so vast and complicated, and bristling with such thorny

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problems, no one writer can be said to have pronounced the final word. But in this comprehensive and admirably arranged study Mlle Danthine has offered to all who are interested in the subject an inexhaustible fund of material and clearly presented information which scholars will find indispensable, for all will gladly acknowledge the great merits of this remarkable work.

E. DOUGLAS VAN BUREN.

SARDINIAN STUDIES. By Members of the Le Play Society. Published by the Society 1938. pp. 60 and 12 plates. 2s.

Sardinia is rich in exceptionally well preserved and imposing prehistoric monuments, and has moreover yielded a vast number of curious bronze figurines of the warriors who built some of the defensive constructions. To British archaeologists the monuments are of especial interest owing to the remarkable similarities some exhibit to certain groups of Scottish and North Irish remains—the Horned Cairns and the Brochs. A useful English introduction to their closer study, together with notes on the geology, fauna and flora of the island and its later ecclesiastical architecture, is provided by the handy little volume of essays written by members of the Le Play Society who visited Sardinia in 1934.

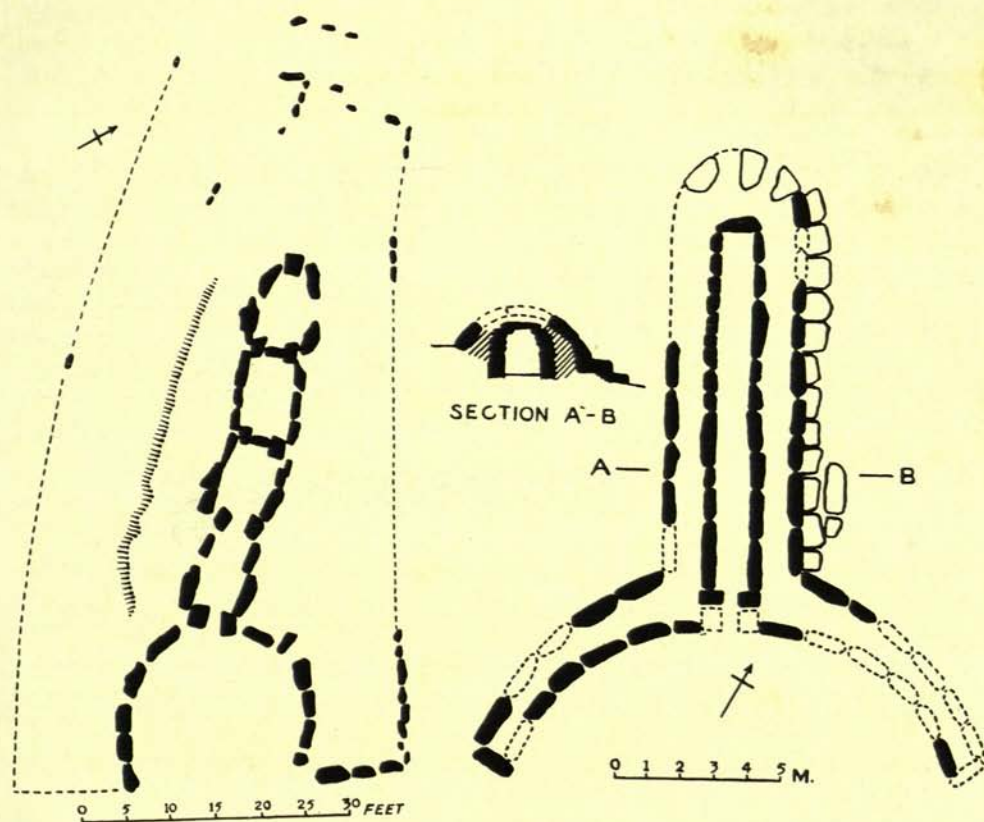
The megalithic gallery-graves, termed locally *tombe di giganti*, or 'Giants' Tombs are extraordinarily like the horned cairns of North Ireland and Scotland as Bryce noted in 1902. But plans are needed to make the comparison intelligible (PLATE I). And then it must be remembered that, while the British tombs are built of rude blocks, the façades and even the chambers of *tombe di giganti* are very often formed of dressed slabs. In classical specimens the 'portal' takes the form of a dressed and carved stele at the base of which a diminutive port-hole opens into the chamber (see PLATE II). This stele is disproportionately high in comparison with the chamber and the rest of the façade. But high portals, composed of undressed monoliths set very close together, are also a feature of the Scottish horned cairns as defined by Bryce. The latter, of course, are standard monuments of the 'Stone Age', but the stones of the Sardinian tombs seem to have been dressed with metal tools.

But in Sardinia, we are told, the *tomba di giganti* developed out of the simple dolmen¹ . . . that is, a large flat stone resting on uprights to cover the place of burial'. I saw a classical 'dolmen' last January beside the railway at Birori² near Macomer. Viewed close up the single table supported by seven uprights conforms perfectly to the usual definition. Examination of the vicinity,

¹ This thesis was worked out in detail by Mackenzie (*Papers of the British School at Rome*, v and vi).

² A plan of the 'dolmen', as in PLATE III, is published in *Bulletino de Paletnologia Italiana*, 1906, xxxii, 268.

PLATE I



PLANS OF MEGALITHIC GALLERY-GRAVES, IRELAND (LEFT) AND SARDINIA (RIGHT)
(see p. 376)

PLATE II



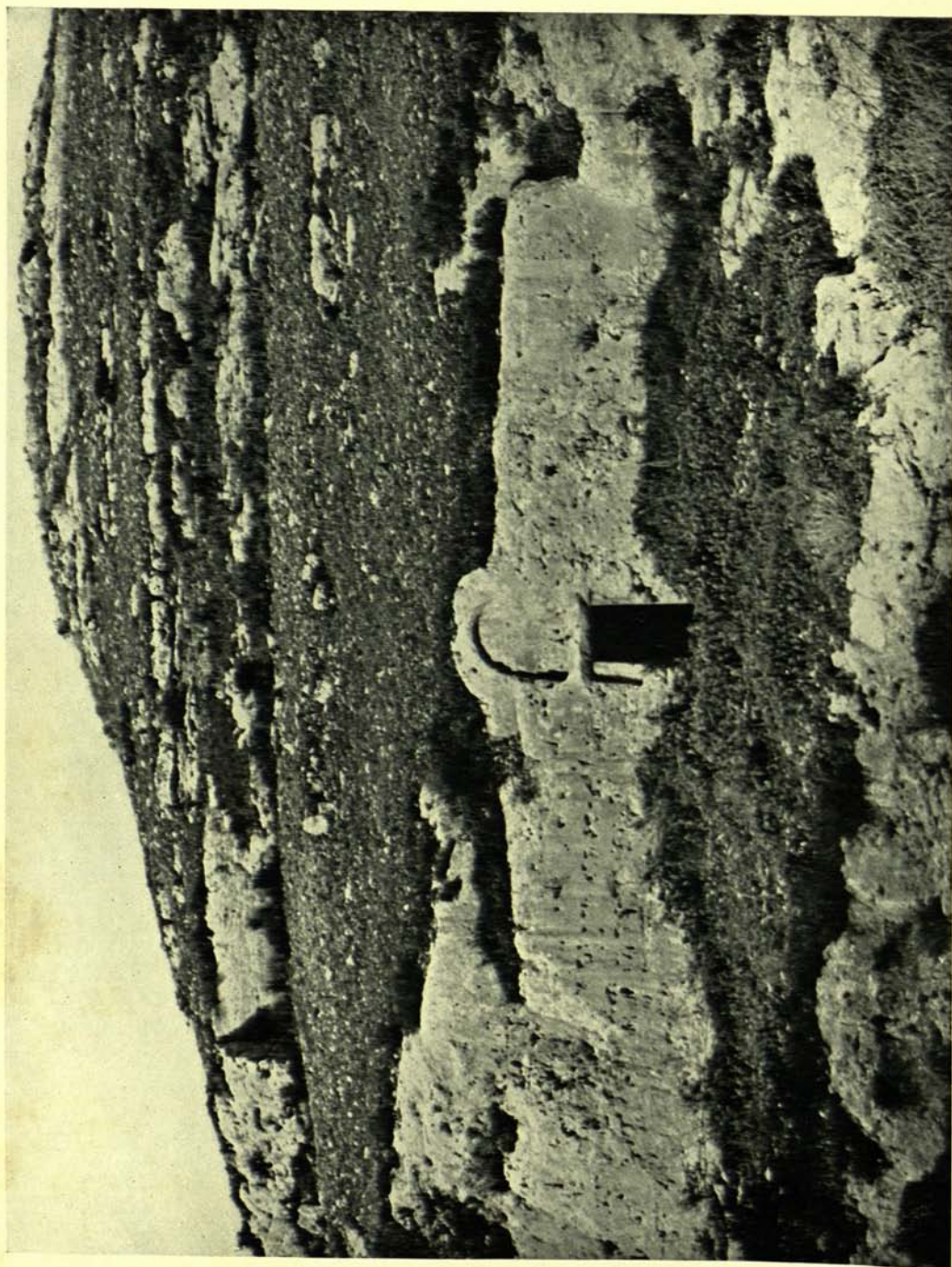
DRESSED SLAB OF 'GIANTS' TOMB', SARDINIA, WITH OPENING INTO CHAMBER (*see* p. 376)

PLATE III



'DOLMEN' AT BIRORI, NEAR MACOMER, SARDINIA (*see p. 376*)

PLATE IV



ROCK-CUT TOMB ('WITCHES' HOUSE') NEAR SASSARI, SARDINIA (see p. 377)

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however, discloses other stones and in particular three set on a semicircle just like the foundations of the façade of a normal *tomba di giganti* (PLATE III). Close by the gallery of such a tomb is preserved complete with five uprights along each side and one capstone still in position. There are vague traces of a third tomb too. It really looks as if this 'dolmen' in any case is just the best preserved section of a similar gallery-grave—in fact of a typical *tomba di giganti*! To assume a priority of the 'dolmen' over the Giants' Tomb on such evidence would seem unjustifiable.

On the other hand some *tombe di giganti* are clearly contemporary with some of the rock-cut tombs, termed locally *domus di gianas*, Witch's Houses. Over the doorway of two such rock-cut tombs near Sassari a faithful reproduction of the stele, that forms the entrance to the built tombs, has been carved. The rock-face on either side of the door has been quarried away and smoothed till it is concave like the built façade on either side of the portal of a Giants' Tomb (PLATE IV). This provides a convincing example of the interchangeability of rock-cut and megalithic chamber-tombs. It would be interesting to plot the distribution of the two types on a geological map—since the method of construction is presumably determined by the nature of the local rock. But the geological map in *Sardinian Studies* is too small for this purpose, and no adequate distribution map of the monuments has even been published—that given by the Le Play Society does not even distinguish between built and rock-cut chambers!

Be that as it may, while some Witches' Houses are Iron Age, others, like those at Anghelu Ruju, go back to Beaker times or earlier. And even these may have the doorways carved, though not with 'stelae'. So some Giants' Tombs too may be Copper Age. Their distribution does indeed approximate to that of the (late) 'Bronze Age' *nuraghe*, but *nuraghe* are too widespread for that to mean much. There is no *nuraghi* at all near the Birori group of tombs. Though the two tombs at Geronna stand on the same plateau as a ruined *nuraghi*, the latter is built over the still more ruined wall of an earlier enclosure.

The *nuraghe* themselves—those towers of dry masonry that from without look so like the Scottish brochs—are adequately described in the book. An illustrated appendix is devoted to the solid-wheeled carts³ still in use in the mountainous district round Abbasanta and Macomer. Like the bullock-carts

³ Illustrations of solid-wheeled carts still in use have been published in *ANTIQUITY*, 1929, III, 340-1 (Sardinia and Spain), and 1931, V, 197-9 (India). Carts with solid wheels of precisely the same type as the Spanish ones were still used in England in the 18th century. Pococke saw them at Penrith in Cumberland in 1760 and thus described them: 'The wheels and Axel trees of their carts turn together, and the wheel consists of three pieces of wood; a small segment of a circle being cut out of the two side pieces and a little from the middle piece' (*Tours in Scotland*, by Richard Pococke, Scottish History Society, 1887, vol. I, 36). A solid wheel of this type found in Tindbaek Mose, Denmark, is illustrated in Brønsted's *Danmarks Oldtid*, 1938, fig. 93.—EDITOR.

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of Sindh, they illustrate much the same type of wheel as is represented on the 'Standard' from the 'Royal Tombs' at Ur and contemporary early Sumerian documents. This Oriental type, introduced into the island in prehistoric times, has been preserved, because, we were told, the solid wheels stand the wear on roads of naked jagged rock better than spoked ones. V. GORDON CHILDE.

BRITISH CORACLES AND IRISH CURRAGHS. By JAMES HORNELL.
Bernard Quaritch, 1938. 46 plates and text-figures. 7s 6d.

Mr Hornell's work on the typology and evolution of boats is well known to readers of ANTIQUITY. With this book he places the archaeologist, and all others interested in the survival of the past into the present, deeply in his debt. As happens not infrequently in enquiries of this kind, it is left to one whom the extreme nationalist would label a 'foreigner' to record and interpret cultural features which are unique in Europe. Here, for the first time, is a comprehensive survey of the wicker-work craft of West Britain and Ireland; their distribution, varieties, modes and materials of construction, uses, methods of carrying and propelling, types of paddle and oar, terminology and customs associated with the craft, their history and probable lines of evolution and devolution. An admirable series of photographs and scale-drawings rounds off the study.

The author believes that the British coracle, usually round or oval in shape, is a far older form than the boat-shaped curragh, which was 'evolved in Celtic Britain from a mixed ancestry, its form modelled upon that of ancient plank-built boats'. The former, he holds, is derived from the same common source as the *quffah* of Iraq, a chapter on which is appended. All Irish vessels of this class, including the oval Boyne type, are built bottom upwards, and it is partly for this reason that Mr Hornell would attribute the Boyne curragh to a process of degradation from a sea-going form. One would like to know what kind of craft was used in pre-Celtic Ireland. Irish archaeology may help to solve this important problem if it can discover curragh-remains in the peats, and extensive excavation of one of the prolific Bann sites is an urgent requirement. Paddles are already known from the Bann valley.

The book has two minor defects consequent upon its origin as a reprint of a series of articles which appeared in *The Mariner's Mirror*, the Quarterly Journal of the Society for Nautical Research, although it must be said that its attractive format conceals its origins. The first defect is that the pagination and figure-numbers are not consecutive, and the second is that the general reader is often held up by technicalities which would no doubt be familiar to members of the Society. A short glossary would have helped the not inconsiderable number of readers who have no first-hand knowledge of boat-building. We

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welcome the news that the surviving types of Irish fishing craft, apart from the curraghs, are being studied, and we strongly recommend this book not only for its own sake but also because, as Sir Geoffrey Callender tells us in his introduction, any profit on its sale goes towards the cost of the Irish survey.

ESTYN EVANS.

THE DISCOVERY OF MAN ; the story of the inquiry into human origins.

By STANLEY CASSON. *Hamish Hamilton*, 1939. pp. 339. 12s 6d.

Histories of archaeology and anthropology may easily become unreadable catalogues, where great men and small are each allotted the same amount of space. This is not that sort of book (none of Mr Casson's are). It is at its best when the author is writing the history of his own special branch of archaeology ; but it is readable even when he is describing the rather dreary history of discovery in South America. Certain new high lights emerge of which we had never even heard. The fascinating story of Cyriac of Ancona (one of them) is admirably told. We are not quite sure that the story of Boucher de Perthes is quite fully or accurately given, though Mr Casson's account is clearly correct in most parts. Sir John Evans and his colleagues *were*, surely, satisfied that, in spite of occasional salting, genuine implements *had* been found in position in the Somme gravels. Some years ago Sir Arthur Evans mentioned to me in conversation that he had himself, as a boy, accompanied his father on one of his expeditions to France. Impressed by the interest of the living link thus provided with the Heroic Age of modern archaeology, I asked Sir Arthur to set down the chief facts in writing, and this he kindly did, allowing me at the same time to publish them. In a letter dated 18 May 1936 he describes the extent of his 'personal knowledge of the chapter of palaeolithic discovery that began in 1858. In that year', he wrote, 'encouraged by Dr Falconer's good opinion of Boucher de Perthes' implements . . . my father (Sir John Evans) joined Professor Prestwich in actual researches in the S. Acheul and Amiens gravels ; on which occasion, as you may remember, they actually found a flint implement *in situ* 17 feet down. I was then only a child of 7, but I recall the domestic reactions and both Falconer and Prestwich very well. It was only in January 1865 that I was permitted to accompany my father in a journey to France. Our first quarters, as I remember well, was Béthune, and we made a round of the principal Somme valley sites, and saw the French people concerned there and in Paris ; thence going down south to see Marquis de Vibraye's wonderful collection of cave specimens. I also remember the Abbé Bourgeois' collections at Pontlevay. I was then 13½, but I quite entered into the atmosphere of those days'.

I have kept this letter till now, waiting for a suitable occasion to print it, and I do so now on account of its great interest, and not so much in criticism of

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Mr Casson's account as to supplement it. It is pleasant to record such a link with those far-off days, and to know that the great archaeologist who remembers them is still amongst us. Mr Casson describes in chapter 5 (Great Discoveries), Sir Arthur Evans' own role in the making of archaeological history, of which it may truthfully be said that in archaeology it is the finest single-handed achievement of all time.

The author plainly sees—what so many overlook—that modern archaeology dates from the moment when *things* were made to tell their own story, to reconstruct the past without regard to documents. I think he might have enlarged a little more upon this interesting theme; the change-over in attitude marks the beginning of scientific archaeology as opposed to antiquarianism. I am still a little puzzled about it myself—how and when did it occur? Generally speaking, about the beginning of the 19th century, of course, but it seems to have crept in silently. Perhaps here again it was Boucher de Perthes' discoveries that were the first outstanding instance of the new method of approach. In England Sir Richard Colt Hoare (who should be mentioned—he was a far greater man than the collector William Greenwell) very nearly attained the modern outlook; he was, I think, the first to publish a stratified section of an earthwork (Wansdyke) and to draw conclusions from it; and he certainly drew conclusions from his barrow-finds and British villages.

Aubrey was, however, a good and indefatigable field archaeologist in the century before Stukeley; his observations of earthworks, recorded in the still unpublished *Monumenta Britannica*, prove this. He was also the discoverer of Avebury! The plan of it which he has left is by no means useless, though neither so full or on so large a scale as Stukeley's.

Schliemann is rightly given the place of honour which is his due. Speaking once with the veteran Professor Sayce I asked him—who had watched the whole story of modern archaeology develop, 'et cujus pars magna fuit'—with whom, in his opinion, it had begun; he replied, without a moment's hesitation, 'with Schliemann'.

After Schliemann, and in another field, General Pitt Rivers was the outstanding figure. He surely deserves more than the single and not very adequate paragraph on p. 278; for it was he who introduced scientific method into the excavation of prehistoric sites in Europe. Pitt Rivers' influence has affected every living excavator in Britain and he is still (as in my day) respected by members of the 'younger school', who are not in any generation prone to be tolerant of their elders (and often with justification). So excellent were his methods of record that fresh information can still be extracted from his excavations as new problems arise, and new demands are made.

I conclude with a hearty commendation of an interesting and valuable book, and the usual irritating list of quite minor points.

REVIEWS

Page 182, Ightham is situated not 'in' but *near* 'the rolling chalk hills of Kent'. It stands on the Lower Greensand.

Pages 287, 288. The evidential value of the Kentish eoliths is NIL. Well attested *palaeolithic* implements have been found in the highest plateau gravels of Kent, and prove the existence of man at the time when those, the oldest, gravels were deposited. The game being thus won by playing the higher card, the lower ceases to be of any value as evidence.

Page 300. The palaeolithic is the period of hunting rather than of food-collecting, which is characteristic rather of the marginal Europeans of mesolithic times.

Page 317. One certified discovery *appears* to have been made, but it is still not published, so far as I am aware, and until then judgment must be suspended (see *ANTIQUITY* 1938, XII, 232).

Page 320. The best modern opinion inclines to the view that America was first reached from Asia *via* the Aleutian Islands rather than across the Behring Strait.

Page 326. Omit France.

O.G.S.C.

HISTORY AND ROMANCE IN GRAECO-ORIENTAL LITERATURE.

By MARTIN BRAUN. *Basil Blackwell*, 1938. pp. 106. 7s 6d.

This is an interesting treatise on that curious type of literature, half folk-literature and half legend and history, which grew out of the main body of Greek and Oriental writing. As literature it has almost no value; as a special and inner view of the Oriental mind it is of importance. The stories of Semiramis and Ninus are analyzed by the author with care and scholarship. The interaction in Greek and oriental story-telling of the original myth is explained. Sesostris, the Egyptian national hero, is shown developing as a nationalist saga when Egypt was under Persian rule. Fragments of Greek history creep into the general story. Nectanebus, the last Egyptian native Pharaoh, was another national hero of story and legend.

Moses also figures as an Egyptian hero, originally a priest of Heliopolis. Finally Alexander is discussed—Alexander about whom more legends accumulated than of any other person in history. The author also deals with the Potiphar story and its ramifications in Jewish and Hellenistic legend.

The work is an interesting one, for it shows literature in decay and legend in the making.

S. CASSON.

AIGINA. By GABRIEL WELTER. *Berlin: Archaeologisches Institut des deutschen Reiches*, 1938. pp. 134, and 87 figures. RM. 5.10 in England.

This is one of an excellent if small series sponsored by the German Institute. 'Priene' is a similar handbook. Gabriel Welter has long established himself

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as the leading authority on the island of Aegina, and his patient excavations have contributed largely to its prehistory. This book, excellently produced and illustrated, gives the salient points in Aeginetan political history and art, from the Neolithic Age to the present day. Aeginetan neolithic pottery seems to stand in a class apart from other Greek neolithic wares. It is a fine painted ware, and the shapes are aesthetically good, as in all Greek neolithic pottery, but neither conform closely to Peloponnesian or Thessalian standards and types.

The Early and Middle Bronze Age, on the other hand, is that of Greece as a whole, and between the Neolithic and the Early Bronze Age there is the break in culture universally perceptible in Greece. Aegina is particularly rich in Middle Bronze Age finds, one vase-fragment with a representation of a human figure being a remarkable addition to our ceramic knowledge of that period. Minoan influences are perceptible later, and later still fine examples of pure 'Palace Style' Mycenaean ware are found. The Mycenaean pottery continues without a break to the close of the Bronze Age. In the 13th century a hill-settlement was established on Mount Oros, the highest point in the island which Dr Welter attributes to invading Dryopes. Soon after it was established, at a date suggested as 1230 B.C., it was destroyed, together with the main town in the island, and Aegina appears to have been almost completely denuded of inhabitants.

Dr Welter deals with the recorded history of the island in detail and gives a most convenient account of the Aphaia sculptures. He concludes with a useful chronology of the history of the island and a bibliography, a list of cults and artists, and a useful chart of ceramic imports. Altogether an ideal handbook both for visitors and for archaeologists.

S. CASSON.

THE ART OF EARLY WRITING, with special reference to the Cuneiform system. By CUMBERLAND CLARK. pp. 151, and 16 plates. *The Mitre Press*, 1938. 6s.

The author of this little book is an enthusiastic collector of cuneiform tablets who desires to communicate his enthusiasm 'to others who up to now have no knowledge of a most engrossing and inspiring branch of study'.

Early in the book (p. 15) we find the remarkable statement that the Sumerians, when they settled in Mesopotamia, brought with them 'a picture-writing which they had impressed upon flat stones and papyrus leaves'. It would be interesting to know in what country and at what period of civilization the Sumerians had learnt the use of the papyrus-plant as a source of writing material. In chapter v the author discusses the origin of the Sumerians, and with the enthusiasm which inspired him to collect cuneiform records he adopts Waddell's theories of the Aryan origin of the Sumerians and their language. He also

REVIEWS

ascribes the origin of Egyptian civilization to 'Sumerian pioneer efforts', (p. 57). A little farther on we read that 'in this country originated the story of Noah and of the Babylonian hero, Gilgamesh, both of whom survived the flooding of the world; and it is not difficult to understand how or why'. We learn on p. 131 the reason for this rather perplexing statement, since we are told there that in the Mesopotamian 'Deluge Tablet' is to be found the Babylonian account of the Flood, 'in which their hero Gilgamesh has very much the same experiences as Noah'. Presumably the author has confused Gilgamesh with his ancestor Ut-napishtim, the Babylonian Noah, the only man who, according to Babylonian tradition, survived the Deluge.

The man in the street, for whom this book is intended, is not likely to be in a position to know that Waddell's theories are extremely precarious, or that there is no evidence whatever that the Sumerians used papyrus for writing material, or that Gilgamesh never passed through the Deluge, so far as our present state of knowledge goes.

It is to be regretted that the author's enthusiasm has not been a little better informed with elementary knowledge. His intentions are admirable, and there is much useful information in his book, but what is reliable is so intermingled with misleading statements that it is impossible to recommend the book to the class of reader for whom it is intended.

S. H. HOOKE.

A REPORT ON THE MUSEUMS AND ART GALLERIES OF THE BRITISH ISLES (other than the National Museums). By S. F. MARKHAM to the Carnegie United Kingdom Trustees. *Edinburgh: T. and A. Constable, 1938. pp. 179 and 24 plates.*

The Carnegie Trustees have again earned our gratitude by the publication of this comprehensive Report, which enables us to judge of the progress made by Museums as a whole during the ten years which have passed since the appearance of the Report by Sir Henry Miers.

Undoubted progress has been made; there are more good museums and more museums striving to be good. It is also apparent that the public, when given a chance, appreciates good service in museums as well as in other spheres.

It is cheering to read that more museum authorities have realized their threefold duties of conservation, research and education. Perhaps the most noticeable improvement is in showmanship. Apart from such costly features as the diorama, there is a widespread and successful effort to display ordinary museum objects to better advantage. Curators are realizing what the proprietors of fashionable hat-shops learned years ago, that the spectator is merely dazed by the overpowering effect of too-crowded show cases, that attention can be called to a particular object by placing it in reasonable isolation and that a good

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light is necessary for objects to be seen. It is also being realized that the museum visitor has the right to be able to learn what he is looking at (without the tiresome necessity of buying a catalogue) by means of legible and concise descriptive cards and labels.

A striking example of old and new methods at the Liverpool Public Museums is given on plate 17. The old, with its heavy shelves, cluttered with far too many objects, and the new with its pleasing and dignified arrangement on tiers of rectangular blocks. (But even with this admirable arrangement, there is not a single label—nothing to tell the visitor what the objects are, what they mean or where they come from).

Progress there has certainly been, but there is another side to the picture. The majority of museums are suffering from most serious handicaps—inadequate finances, unsuitable buildings, insufficient staff, or all of these. How can such institutions, however hardworking and keen their staffs, really carry out the purposes of a museum? Again there is a residuum of really bad museums, those which have given up the struggle, with curators too indolent to attempt any improvement, controlled by committees ignorant of the very purposes for which museums exist.

Perhaps the most interesting, as well as the most controversial, section of the report is that which gives Mr Markham's proposals for the improvement of the unfortunate, and the elimination of the bad, museums. It is no less than the nationalization of all museums. This proposal is certainly revolutionary; it will arouse much opposition; it can hardly be considered practicable at the present time—but it leaves your reviewer with a feeling of hope. Speaking only for himself, he would endure any number of the inevitable Government Inspectors, he would fill in any number of the inevitable departmental forms, if only he had the certainty that his museum could 'fulfil its destiny' uncramped and unhindered by the heartbreaking restrictions that present conditions impose on all but the most fortunate of museums.

The report can be obtained free of charge by anyone interested who will apply to Comely Park House, Dunfermline.

CHARLES D. DREW.

Antiquity

A Quarterly Review of Archaeology

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Editorial Notes

THE European War, which has already affected the lives and outlook of millions of people, presents problems to the Editors of ANTIQUITY, who for various reasons have had to consider what course they should take. At first the difficulties which were envisaged pointed to the present (December) number having to be the last of the series which began in March 1927, but after the most careful survey of all the possibilities and probabilities they decided to take courage, and at any rate plan for the year 1940. At the same time they are well aware there may be a risk of their hopes not being entirely realized.



This decision having been made, we would like to thank those of our Subscribers who quite spontaneously have written expressing in the most friendly terms their hope that ANTIQUITY will continue. They press the view that it occupies a place entirely its own, and that every endeavour should be made to avoid the gap which its end would leave for all who are interested in the subjects with which it deals. Admitted that such expressions are few compared with the number of our Subscribers, but we believe that *the majority of them will expect us to make the endeavour.*

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One of the staunchest of our friends says it is of 'paramount importance that all *foci* of Culture and Learning should be kept alive' during the present troubles, and with this we are in entire agreement. It seems to us it is right that ANTIQUITY should play its part in this effort, so that when Europe breathes freely once more, we may continue, with unabated strength, to represent what we consider an essential contribution to Learning and Progress.



Our reliance on the continued help and interest of those who have so wonderfully supported us since ANTIQUITY was established enables us to take this stand. It is only that which gives us courage; without it we quite certainly could not continue publication. We should very much dislike to make any difference in the standard, size, or the quality of the illustrations which have won fame for our Review; or change in any way the principles which have been maintained and have brought it to the position which it holds. The difficulties which now affect the production of such a journal as ANTIQUITY present financial problems which have to be carefully weighed. It is here that our Subscribers can fortify our decision, and this assistance can be given in more than one way.



First, we ask for early attention to the circular which will be found inserted in the present number (with the exception of copies posted to those who already have standing orders for payment of subscription). Those who are with us in the wish to add our service to Culture and Learning will show it by sending the subscription for the year 1940 as soon as it may be convenient. We do not as a rule post a reminder for payment until June, but under existing conditions it is most important we should know fairly closely how many copies of each number will be required. The restrictions on the use of paper make this very necessary.

Secondly, those who hitherto have not made use of payment by Bank Order would be giving the best help by doing so, and filling in that part of our circular. They save themselves trouble, and ourselves time and expense through not having to post statements.

EDITORIAL NOTES

From those who already have a standing order, but who for sufficient reason may need to consider its withdrawal (and we are of course fully alive to the pressure of the taxation which so many will feel), notice to this effect will be appreciated. At the same time we trust that ANTIQUITY will not be chosen as one of the victims of the recent Budget.

The courses mentioned will enable us to know approximately what our circulation for 1940 will be, and to avoid posting copies of the March number which are really not desired, though we earnestly hope there will not be many cancelled.



We have entered into these details in order to know where we stand. For our part, if we can meet our bills—which are by no means light—it is all we ask. The work which production involves is considerable, but so long as ANTIQUITY lives we are prepared for that, and shall do all in our power to keep the flag flying.

In due time the World will regain sanity, and much that makes life worth while will return, though we are aware that the inevitable outcome of the War will mean curtailment of many quiet pleasures.



The present number of ANTIQUITY has been planned so that it will include the usual particulars of the contents of our Volume, and an Index which is as complete as those which have preceded it. The only difference in size is a few pages less of text, which will balance the special cost of the additions mentioned.



One of the considerations which give some anxiety is the flow of contributions of articles and notes. Many writers for ANTIQUITY are on Active Service and have no present opportunity for such work. Material is essential for the life of the Review, and we hope it will continue to be offered for publication.

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Anglo-Saxon Ship-burial in Suffolk

It was our intention, as we announced in September, to make the present number one of special importance by devoting it to articles dealing with the wonderful discovery earlier this year of the Saxon ship-burial and its treasure at Sutton Hoo, near Woodbridge, in Suffolk. The outbreak of War prevented this, but we now hope to publish several such articles in March. That number will therefore contain the first full and authoritative description of the greatest archaeological discovery ever made in Britain.



It is already well known that Mrs E. M. Pretty, the owner of the burial-site, has most generously presented the whole of the finds to the Nation. Eventually they will form a special exhibit in the British Museum. Mrs Pretty's gift means therefore that for all time the treasure of Sutton Hoo can be studied as a whole, and these magnificent examples of Saxon workmanship examined under the best conditions.

We take the opportunity of thanking those concerned with this unprecedented revelation of Saxon days, both for this exclusive privilege thus granted to us, and for their forbearance in consequence of the inevitable postponement. The articles will be illustrated by photographs of the objects of gold, silver and bronze, and other materials, together with others taken by one of the Editors during the excavations.

Rock-paintings from the Libyan Desert

An Appendix to Dr H. A. Winkler's
'Rock-drawings of Southern Upper Egypt II'¹

by R. F. PEEL

THE two volumes of rock-drawings from southern Upper Egypt collected by Dr H. A. Winkler, and published by the Egypt Exploration Society, together form a work of the utmost interest and importance to all interested in the archaeology and ethnology of North Africa. In the first volume, published in 1938,² Dr Winkler included a selection of the material collected from the deserts east of the Nile and from the Nile valley itself. In the second volume, just published, the drawings and paintings are all from the deserts west of the Nile and cover three main regions: first the edges of the Nile valley itself from Qena to Aswan; secondly the regions between the Nile and Kharga; and thirdly certain parts of the central Libyan Desert towards the extreme south-western frontiers of Egypt, in particular the mountain 'desert oasis' of Gebel 'Uweināt.

To visit this third and most inaccessible region Dr Winkler joined for a month the desert expedition organized early in 1938 by Major R. A. Bagnold on the one hand, and Mr O. H. Myers and the Egypt Exploration Society on the other, an expedition which the writer accompanied as surveyor and geographer. A general account of the aims and accomplishments of this expedition was given at a meeting of the Royal Geographical Society in January 1939, and will be found in the *Journal of that Society* for April 1939.³ In brief, the combined expedition travelled from Armant in the Nile valley to Kharga, and thence south-westwards towards the Egyptian frontier until the great sandstone plateau of the Gilf Kebir was reached. A month was spent in detailed work on and around this plateau, the work including exploration and survey, archaeology, and a variety of geophysical and geological investigations. During a second month, in which Dr Winkler joined the party, the expedition proceeded to 'Uweināt, situated on the triple frontier

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of Egypt, the Sudan and Italian Libya. The archaeological section of the party, comprising Mr O. H. Myers, Dr H. A. Winkler, Mr T. Gray and their assistants spent the greater part of the month in detailed work around the massif. Major Bagnold and the writer however made a separate journey during this second month, northwards along the western side of the Gilf Kebir and into the Sand Sea area to the north of it; and during this journey two rock-painting sites, which Dr Winkler was unable to reach, were visited and studied. One of these sites was previously known but, so far as the writer is aware, has never been fully published; the other was a new discovery. It therefore seems opportune at the time when Dr Winkler's account of the material at 'Uweinat, and at one site in the southern Gilf Kebir, has just appeared, to add this brief summary of the nature of the material at these two northern sites.

PREVIOUS DISCOVERIES AND LITERATURE

Rock-drawings, comprising both engravings and paintings, have long been known from a number of localities in the Libyan Desert. Engravings were seen and recorded at 'Uweinat by the first explorer to reach it, A. M. Hassanein Bey, in 1923,⁴ and almost every subsequent visitor has added to the list of known sites. Prince Kemal el-Dine, visiting the mountain in 1925-26, published new examples,⁵ which were examined and discussed by H. Breuil.⁶ Breuil had previously written on the examples found by Hassanein and pointed out the similarity of the drawings to those of the early Bushmen.⁷ Major R. A. Bagnold recorded drawings from the northern wadis of 'Uweinat in 1930 and 1932,⁸ and L. E. de Almasy found many more in the course of several visits to the massif. On one of these visits, in 1933, Almasy discovered the rich series of paintings in the grottoes of Ain Dua, on the southern side of the mountain,⁹ a series which has since been studied in detail and published by L. di Caporiacco and P. Graziosi.¹⁰ Further descriptions of the 'Uweinat drawings have been published by W. B. Kennedy Shaw in *ANTIQUITY*¹¹ and the same writer has also published a general account of the massif.¹² An expedition sent out by the Cairo newspaper 'Ahram' in 1934 made a large collection of drawings and photographs of the 'Uweinat series, but this material has apparently never been published. Finally in 1938 Dr Winkler devoted a fortnight to the detailed re-examination of many of the known sites, and discovered a number of new ones, particularly in Karkur Talh, the large northern wadi where the bulk of the previous discoveries had been made, but

ROCK-PAINTINGS FROM THE LIBYAN DESERT

also in the southern wadi Karkur Murr following the discovery by Bagnold of a fine painted cave at the head of that wadi.¹³

Although in actual numbers 'Uweinat has proved by far the most prolific region for rock-drawings many discoveries have been made elsewhere. Almasy found interesting paintings, in the same style as those at 'Uweinat, at Gebel Kissu, an isolated granite mountain some 15 miles south of 'Uweinat, and also a few examples at Gebel Arkenu, a short distance to the west.¹⁴ In 1932 Bagnold recorded similar paintings from Yerguehda Hill, a small basalt cone some 60 miles south-southwest of 'Uweinat.¹⁵ P. A. Clayton, of the Egyptian Surveys, while engaged in survey reconnaissance in 1931, discovered faint engravings of giraffes in a small wadi on the western side of the Gilf Kebir in Lat. $23^{\circ}35'$ N. Long. $25^{\circ}15'$ E,¹⁶ a wadi later named by Almasy Wadi Sora (Wadi Sura; Wadi es-Suwar in Winkler, 1939). Exploring this locality in 1933 Almasy found other engravings, and nearby two caves rich in paintings, some of which he reproduced in his book.¹⁷ This site was also visited by the 'Ahram' expedition. Finally, in the course of his long expedition of 1935, W. B. Kennedy Shaw recorded a new painted cave found by R. McEuen in a wadi near the southern extremity of the Gilf Kebir.¹⁸ This site was visited by Winkler in 1938, and is recorded in his recent book as 'site 83'.

Discoveries in regions surrounding this central part of the desert have been too numerous to record in full. Mention may just be made of the early discoveries of engravings at Gebel Tageru, Burg el-Tuyer, and to the north of Merga by W. B. K. Shaw and D. Newbold in 1927,¹⁹ and of the material from northern Darfur recently published by A. J. Arkell.²⁰ Paintings similar in style to those at 'Uweinat were found in the hills east of Ennedi by Bagnold in 1932,²¹ and have more recently been found in large numbers in the Ennedi Highlands by French explorers.²² L. Frobenius has published various examples from different parts of the desert,²³ and the sites listed above have been discussed in many general works by such authorities as R. Vaufrey,²⁴ and L. Joleaud.²⁵ Recent years have witnessed the discovery of many sites in the section of the Libyan Desert under Italian control, but there is no space to refer to them all in a short article; much less to attempt any list of the sites discovered in the French Sahara. Even the brief summary given above however, will indicate something of the profusion of the material already gathered from lands which less than twenty years ago were almost completely unexplored, and are still very imperfectly known.

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SITES AT THE GILF KEBIR, 1938

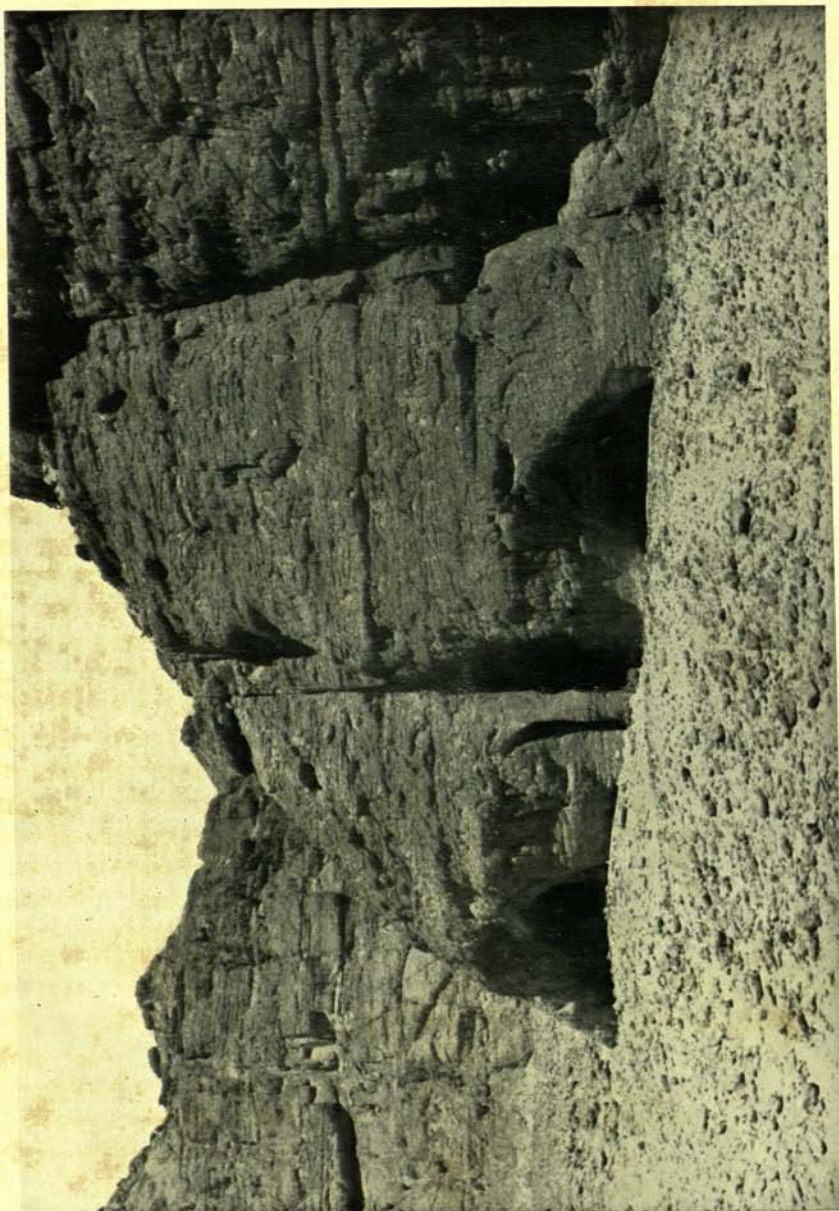
From the above account it will be noted that two sites of rock-drawings (paintings) were known at the Gilf Kebir prior to 1938; the caves of Wadi Sora on the west, and the abri discovered by McEuen and Shaw to the south. In 1938 our first month was spent on the eastern side of the plateau, and a careful exploration was made of almost all the wadis on that side of the southern part of the plateau, together with a considerable portion of its surface. Archaeological material in the form of artifacts, ancient dwelling sites, ancient tracks, stone circles, etc., was found almost everywhere in great profusion, but no trace of rock-engravings or paintings could be discovered. This absence of drawings from what was obviously a well-frequented region in early Palaeolithic and Neolithic times is perhaps largely to be explained by the nature of the rocks. The Nubian sandstones here are prevailingly false-bedded and relatively soft. Smooth faces are rare; caves and abris almost entirely absent. There are few places inviting artistic endeavour; fewer still where it would have any chance of survival. From the general circumstances indeed it seems probable that our failure to find any drawings along the eastern side of the Gilf is to be explained rather on the theory that drawings were originally present but have been destroyed, than that the ancient inhabitants never attempted to draw in this region, although drawing and painting so freely at 'Uweinat.

It is unnecessary to give a detailed description here of the work done at 'Uweinat, or of the new sites found there, as Dr Winkler has given full information in his book. The remainder of this paper will therefore be devoted to a brief account of the two sites in the northern part of the Gilf Kebir, visited in the latter part of the expedition by Major Bagnold and the writer. The first of these was the Wadi Sora site, first discovered by Almasy; the second was a new discovery made in the upper part of the Wadi Abd el Melik.

WADI SORA (PLATES I-II)

As indicated by Almasy in his book describing the discovery,²⁶ the paintings at Wadi Sora occur on the walls of three or four caves situated at the head of a short amphitheatre-like wadi some 3 miles south of the entrance to the main wadi. We found three such caves, but were only able to distinguish paintings in two of them. From their shapes, and other considerations, it seems probable that springs, issuing from the base of the cliffs, were instrumental in forming these caves. They are cut in sandstones rather different from the typical Nubian

PLATE I



PAINTED CAVES IN WADI SORA GILF KEBIR (see p. 392)

PLATE II



TYPICAL PAINTINGS IN THE NORTHERN CAVE, WADI SORA

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beds ; sandstones of a darker brown, and more massive in structure, which appear to dip eastwards under the typical Nubian beds that overlie them. As the writer has suggested elsewhere,²⁷ these beds may relate to the Palaeozoic sandstones distinguished by K. S. Sandford further west.²⁸ The point is immaterial in the present context, but the local presence of these massive sandstones, which are very like those in which the painted shelters of 'Uweinat are cut, would appear to have an immediate significance in explaining the presence in Wadi Sora of large abris containing paintings, while these features are so rare elsewhere around the Gifl Kebir.

Of the two caves found to contain paintings, that to the south (the right-hand one as one enters the wadi), has the smaller number and may conveniently be described first. As in both caves, the walls and ceiling have scaled badly, and only one group of paintings remains intact. About two dozen figures are shown, all of men and cattle, and all in dark red and white. The style is uniform and distinctive : it is that named by Winkler the style of 'balanced exaggeration' which is found at 'Uweinat, at Kissu, in the south of the Gifl Kebir, and in the Ennedi examples described by Passemard. The men have broad shoulders, narrow waists, triangular torsoes, exaggerated rounded hips, long and tapering legs and arms. Feet and hands are rarely shown, and the head is represented only by a round blob, sometimes however extended upwards into a sort of peak and having a beak-like projection in front. Almost all the men carry bows, some of single, others of double curve. Many are shooting, and an interesting feature is that some, at least, of the figures seem to be drawing the bow with the left hand. It is difficult to be certain on this point, as the figures may be interpreted either as facing the observer or as having their backs to him. No other weapons are shown, and no traces of clothing or decoration could be distinguished on the figures. They vary in size from about 5 to over 40 cms. tall. The cattle in this cave are drawn in the same general style as the men, having elegant tapering limbs and long thin tails. Horns could not be distinguished, but may have been in white and have faded. Some of the cattle have patterns of white bands on their sides, presumably to represent brindling. One or two smaller animals, which from their shapes appear to be calves, are shown in front of, and partly below the cattle ; and in one case a man is drawn beneath a cow and having one arm stretched out towards its udder. The udder is shown large and clear between the animal's legs. Cattle are drawn about 25 cms. long and 20 cms. tall.

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In both subject matter and style, these paintings are essentially similar to many of those at 'Uweinat and the other southern sites, and are undoubtedly the work of the same people ; an early people of cattle-breeders whose life apparently centred around the cattle which they represented with such care, but who still used the bow and hunted game. Winkler has applied the term 'Autochthonous Mountain Dwellers' to these peoples ; a name which invites some criticism since practically none of the Libyan Desert is truly mountainous and the term 'Autochthonous' has implications which cannot be proved in this connexion. This is too broad a subject to discuss here however, and it must suffice to state that the paintings discussed from Wadi Sora are so essentially similar to those at 'Uweinat that they can unhesitatingly be ascribed to the same people. (FIG. 1).

The second painted cave contains a greater number of specimens, and some differences of style. Animals are shown, such as a few cattle, ostrich, dog, and ? giraffe, but the bulk of the paintings are of men. Owing to the extensive flaking of the sandstone and the crowding and superposition of the figures it is difficult to give any comprehensive account in a short space. Well over a hundred figures can easily be distinguished, although many are damaged, some by scaling of the rock, others by deliberate defacement ; the original number may well have been twice or thrice that figure. This cave stands some twenty yards to the north (left) of the first one.

Among the mass of figures some broad distinctions may be made. The elegant style of the previous cave is rare ; indeed may be said to be absent in its pure form. Most of the figures are rather crudely painted and some partly conventionalized. Very common is a style of male figure standing rather stiffly and facing outwards. Heads are mere round blobs, torsoes thick, limbs clumsy and hips narrow. Feet are indicated, but hands only on the larger specimens. Most of the figures are in dark red, but many, especially the larger ones, have decoration in white ; bands of white around the ankles, wrists, upper arms, and below the knees probably represent armlets, anklets and garters. Some anklets have a fringe. Larger figures occasionally have similar bands around the chest and loins in various patterns. Figures of this type vary in size from about 5 cms. to over 25 cms. tall. (FIG. 2).

Intermingled with these rather stiff and ungainly figures, which occur both singly and in groups, are many small figures in dark red without decoration, but in a variety of positions of movement and often grouped into a definite arrangement. In style these figures

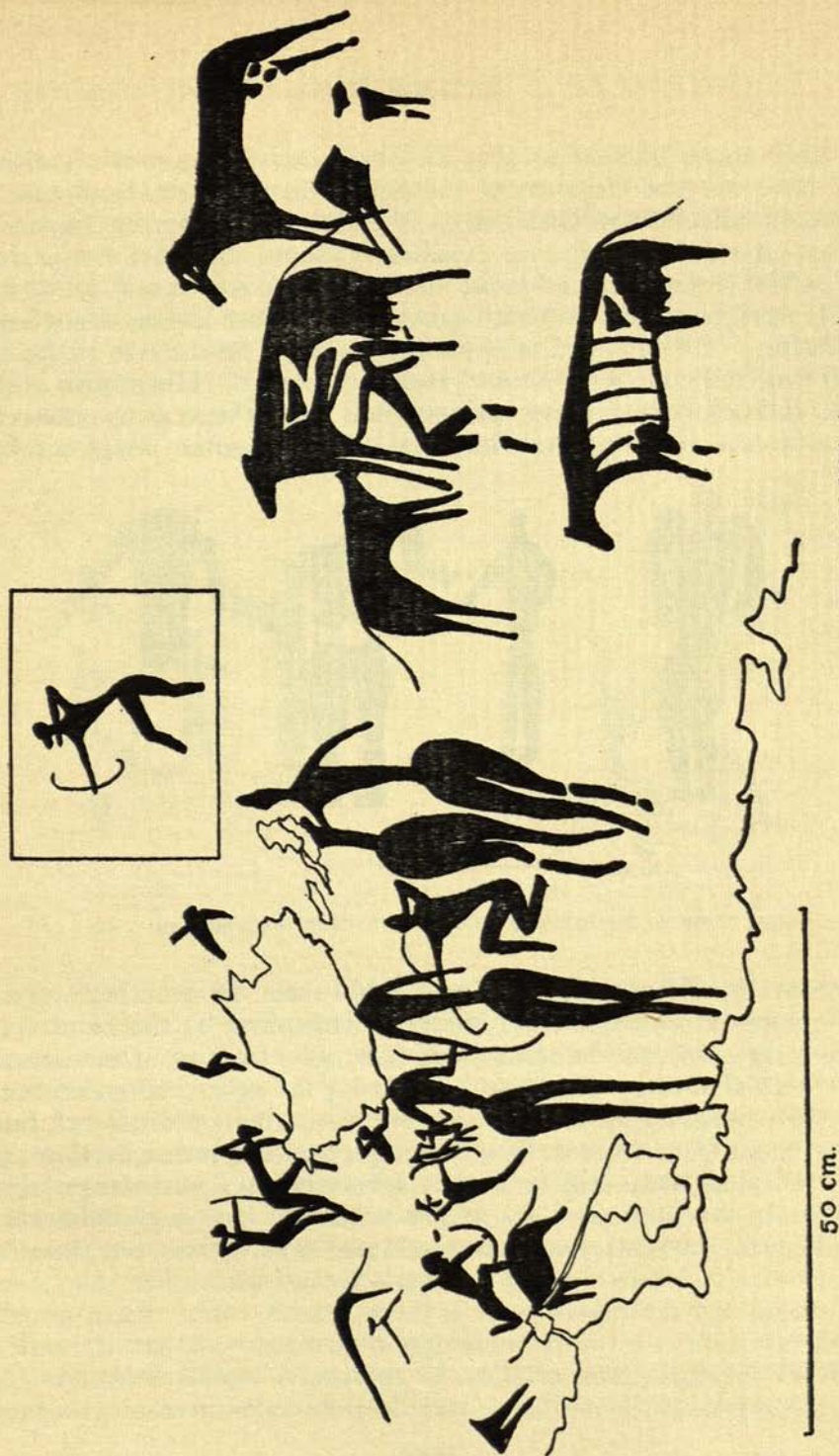


FIG. 1. GROUP OF PAINTINGS IN SOUTHERN (RIGHT-HAND) CAVE, WADI SORA (see p. 393-4)

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approach more closely to that of 'balanced exaggeration', although they have not the elegance of the figures in the right-hand cave, and little anatomical detail is shown. The attitudes and groupings of these figures offer some intriguing puzzles, rendered the more tantalizing by the partial destruction of many of them. In one case (FIG. 3) a man stands upright (side view) with arms raised before him as though giving a blessing. On his head is apparently a large head-dress in the shape of an inverted cone with a tassel hanging from it. His mouth is shown open, and before him stand various small figures with arms outstretched or uplifted. The general impression is of some magico-religious

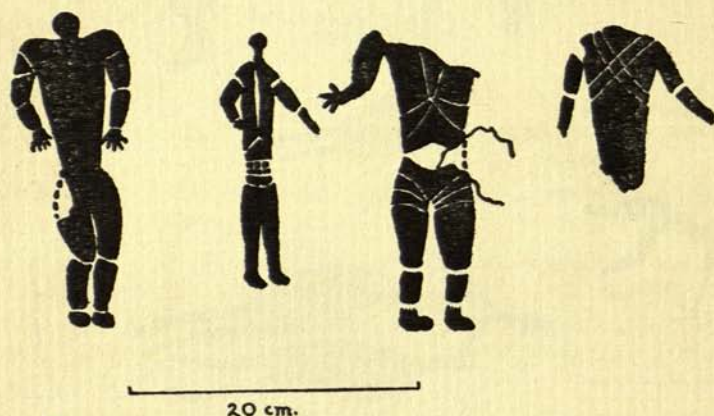


FIG. 2. LARGE DECORATED MALE FIGURES, WADI SORA (see p. 394)

ceremony involving blessing or absolution. In another case one figure, with arm outstretched before it, is leading by the hand another figure most obviously hanging back (FIG. 4). Groups of men running, and others shooting with bows are among the other representations.

Most puzzling of all however are a number of small red figures, seldom more than 10 cms. long, having a small rounded head on a thin neck, rounded body and long thin arms and legs with large feet, the arms being held together above the head, the legs curved backwards. These figures are scattered about, and in all orientations, but the attitude remains the same, and is one difficult to interpret (FIG. 5). Almasry, who copied and described some of these figures, terms them 'nageurs', and the attitude is certainly strikingly reminiscent of that of a swimmer or diver. If this interpretation is correct it would seem to suggest either that pools of water large and deep enough to swim in existed in

ROCK-PAINTINGS FROM THE LIBYAN DESERT

this remote desert area when the cattle people occupied it, or that they had recently come from a well-watered region to one by contrast arid, and that the pictures were inspired by the memory and the contrast; the wish being father to the thought.

Among the other figures a few call for special mention. One isolated painting of a cow, in dark red with white belly and horns, is especially noteworthy for the skill and vigour of its execution (FIG. 6). Other animals represented include the ostrich, giraffe, dog and ? elephant. A few prints of hands were seen, drawn by stencilling round the hand held flat against the wall. No mutilations could be



FIG. 3. WADI SORA



FIG. 4. WADI SORA

distinguished such as are so frequently found in Spanish examples (FIG. 9). Paintings are mostly in dark red; some in white, a few in light red. A small number of figures in yellow are also present. Wherever superposition occurs, the yellow is on top, and seems therefore to be a late development. One group of yellow figures is interesting; a figure in dark red stands between two in yellow, with an arm outstretched to each. The yellow figure on the right is small, and may be a child. The grouping may of course be accidental; but, if not, the group may be intended to represent a union between two different groups, or even a marriage, although there is no indication of sex in any of the figures.

One final type of figure is of particular interest. It is the conventional representation of a man by an oval from which depend two thin legs (FIG. 7). A number of these figures were noted, all in the prevailing

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dark red, and all of similar technique. The style is interesting in that so far as the writer is aware it has not been found elsewhere in the Libyan Desert; the nearest analogy is perhaps the 'figure-of-eight' men found by the Hon. Francis Rodd on the rocks of Air.²⁹ The Wadi Sora figures are not the same; they lack the skirts, arms and other details of the Tuareg examples, and are less stiff in execution, yet there is a slight resemblance: not sufficient for the claim of any connexions, but worth noting with an eye on future discoveries in the intermediate region.

Space forbids a more detailed description of this fine series of paintings, but a few general remarks may be made. The profusion of

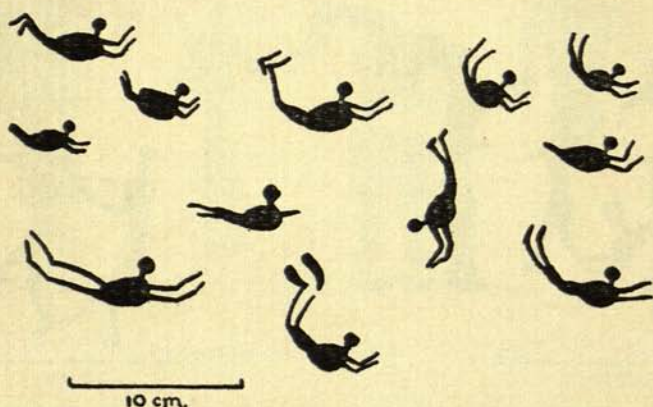


FIG. 3. TYPES OF 'NAGEUR' FIGURES (see p. 396)

the paintings suggests a long period of habitation of this cave, as does the frequent superposition and the variation in styles and subjects. The differences of style are scarcely great enough however to support the idea of occupation by a succession of different peoples, and in general the paintings do not differ in any important respect from those at 'Uweinat. They can, for the most part at any rate, be safely ascribed to the same general group of cattle pastoralists, although the relative absence of cattle among the paintings of this cave is somewhat surprising. Details of dress and decoration are in harmony with those noted by Winkler at 'Uweinat, and add little that is new. It is of interest to note that in addition to the destruction wrought by scaling of the rocks, a large number of the paintings have been deliberately defaced by scraping. Some of these defacements, as judged by patination, look

ROCK-PAINTINGS FROM THE LIBYAN DESERT

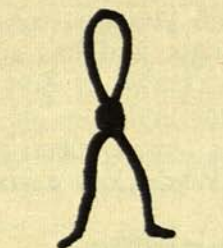
relatively recent, but many are very old. Winkler has suggested³⁰ that such defacements were inspired by spite and resentment, with the implication that the drawings represented certain definite individuals. As against this theory it may be worth noting that here very often whole rows of figures have been uniformly defaced (FIG. 8). which would



20 cm.

FIG. 6

COW IN DARK RED AND WHITE



10 cm.

FIG. 7

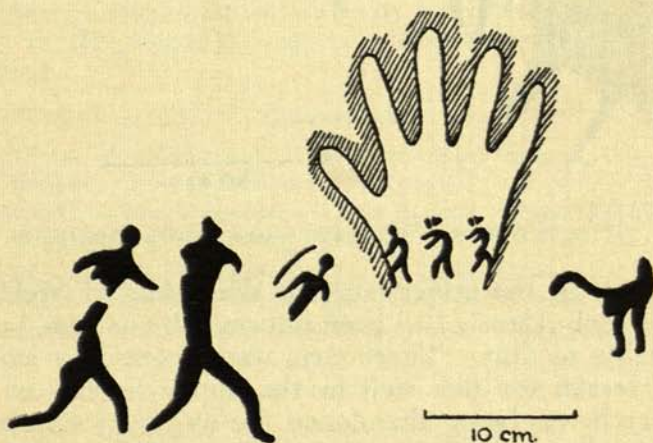
FIGURE-OF-8 DRAWING



10 cm.

FIG. 8

ROW OF DEFACED FIGURES



10 cm.

FIG. 9. HAND OUTLINE, OSTRICH AND HUMAN FIGURES

not be likely if the resentment was only aimed at certain individuals. The defacements are generally limited to the torso and loins, and may have had some motive of sympathetic magic behind them.

Before leaving these caves, a search was made of the floors (without attempting excavation in the absence of implements and expert advice). Crude flakes of silicified sandstone were common, as were many split fragments of quartzite. Pieces of ostrich egg shell, some burnt, others

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bored for beads were present, and in one spot Major Bagnold found a collection of pieces of yellow and red ochre and white clay which may have been the very pigments used by the artists.

WADI ABD EL MELIK

The Wadi Abd el Melik, one of three long wadis in the northern part of the Gilf Kebir which contain trees and grass, was also first discovered by Almasy, in 1932, and has since been claimed by him as the site of the famous 'lost oasis' of Zerzura.³¹ It would be out of place here to discuss the pros and cons of this claim; an admirable summary of the problem has already been given by Major Bagnold.³² In 1938, after studying the Wadi Sora caves, Major Bagnold and the

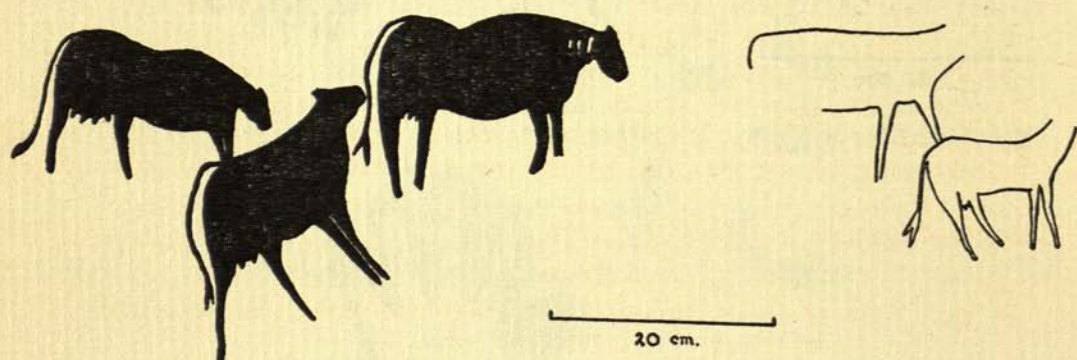


FIG. 10. CATTLE IN DARK RED (LEFT) AND WHITE (RIGHT), WADI ABD EL MELIK

writer proceeded to the upper parts of Wadi Abd el Melik to search for a well of which Almasy had been informed by natives, but which he had been unable to find. Three days were spent in a laborious and unsuccessful search for this well in the upper reaches of the Wadi, but as the search was being abandoned the writer by chance entered a small grotto in the wadi side and noticed very faint paintings on the wall. The grotto is very small—only some ten feet wide, and scarcely deeper—and is situated on the eastern side of the eastern branch of Wadi Abd el Melik, about ten miles above the main fork. Paintings were found on both walls, but all so faint and dust-covered that they had to be moistened from our water-bottles before they were distinct enough to be photographed. All the paintings so shown were of cattle, save for one doubtful dog. They varied in size between 15 and 25 cms. long, and were in dark red, red and white, and white only. In style

ROCK-PAINTINGS FROM THE LIBYAN DESERT

they were almost identical with those in the southern (right-hand) cave at Wadi Sora, and with many 'Uweinat examples; udders being large and painted between the legs, horns absent or small. About ten figures in all were seen, but there were probably many more which shortage of time—and of water—prevented our finding (FIG. 10).

In view of the limited subject-matter, nothing new could be learned from these paintings; their authors were evidently the same cattle-people as those who inhabited 'Uweinat for so long, and the significance of the discovery lies mainly in its evidence of their presence in these northern wadis, and the consequent increase in their known range. In view of the fertility of these northern wadis, and Almasý's claim that they are to be identified with Zerzura, it is probable that a careful search would discover many more sites in this neighbourhood in spite of the rarity of good shelters. It is sincerely to be hoped that fully qualified archaeologists will indeed conduct such researches before the region becomes too frequented and many invaluable paintings are destroyed for ever, for all surviving examples of this early art must be found and studied to the full before any final conclusions can be pronounced as to the identity, origins and relationships of these early cattle-peoples.

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The Relics of Saint Petroc

by CANON G. H. DOBLE

THE incident of the theft of the relics of Saint Petroc from his shrine in Bodmin priory church by a canon of Bodmin in 1177, their removal to the abbey of St.-Méen in Brittany, and their subsequent restoration owing to the vigorous action of Henry II, is well-known. It is briefly referred to by the chroniclers Roger of Hoveden and Benedict of Peterborough. The long *Vita Petroci* in the newly discovered Gotha MS.,¹ however, contains a full and detailed account of the whole affair by a contemporary writer of considerable literary powers. It abounds in lifelike touches, and is one of the most interesting glimpses into the social life of the Middle Ages which we possess. It is here printed for the first time.

The author, Robert de Tautona [or, Tanton], may have accompanied the prior of Bodmin on his voyage to France. He seems to have been a Devon man; North Tawton in Devon was spelt Tauton in the twelfth century. The story was evidently written down soon after the events described, while they were still fresh in the memory, but the words *postea autem archiepiscopo Rotomagensi*, unless they are an interpolation, show that it was after the appointment of Walter of Coutances to the see of Rouen at the end of 1184. On the other hand, it was apparently written before the death of Geoffrey Plantagenet, son of Henry II, on 19 August 1185, as the author mentions him, without saying that he was no longer living.

DE CORPORE SANCTI PETROCI FURTATO ET RESTITUTO

(folio 145)

A certain canon of Bodmin [Bothminia], named Robert of Tauton [or, Tanton], has written the story of how the body of the blessed Petroc was stolen by a certain Martin, a canon of the church of Bodmin, and carried by him into Brittany [Brithannia], and how the same body was brought back again to Bodmin. He says therefore as follows:

For several months we were ignorant of the fact that the body of Petroc, the most holy Confessor of Christ, and our Patron, had been,

¹ Ducal Library at Gotha, M.N. 57. This manuscript, of English (West country) origin, contains nearly 50 lives of English and Cornish saints. It is of the 14th century, but most of the lives it contains are older. The *Vita Petroci* appears to be of the 13th century.

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as a punishment for our sins, sacrilegiously removed and carried to a foreign country.² Having been therefore transported into Brittany to the Church of Saint-Méen,³ according to the good pleasure of God and His most holy confessor its presence was revealed by miracles, and after its sanctity had been thus demonstrated it was exalted [or, elevated] with the greatest honour, and placed [in a shrine] with every mark of reverence. So the report of these doings having spread throughout the whole province, the news reached the ears of the Lord Abbot of St. Michael in Peril of the Sea [*de periculo maris*]. And some of his monks, who had been dispatched into England on the business of the Abbey, being also informed of these things, by God's providence it occurred to them to go to Bartholomew, the Lord Bishop of Exeter, and tell him about it. The Lord Bishop was both amazed and deeply grieved at what he heard. So, desiring to verify the truth of the report, he first questioned Roger, our Lord Prior, who happened at that very moment to have come to see him on business of his own, and who would be obliged to go on afterwards to the King's Court. As the Prior had no certain information on the matter, the Bishop sent the Abbot of St. Nectan [*i.e.* Hartland] and the Abbot of Buckfast [Bucfest'a] and the Lord Prior of Launceston [Lanstonia], with the Prior of Tavistock [Tavistoke] to Bodmin, that the shrine [*teca*] in which that most holy body had first been placed might be opened in their presence, and so the truth might be ascertained. Now when this loss was thus discovered, our grief, our groans and sighs at our misfortune, may be better imagined than described. There are some perhaps who think that events like this are due to chance, against whom Job says, 'nothing is done on the earth without cause' (Job v, 6, Vulg.). And we are to believe that nothing which is done is done except by the ordinance of God, sometimes manifest, sometimes hidden, but always just. For 'all things were made by Him, and without Him was nothing made' (John, I, 3). For in this matter which we have begun to narrate it is evident how beneficently, how mercifully the Lord used the wickedness of this sacrilegious Martin to the glory and honour of S. Petroc. For he who before was barely known and honoured among his own people in Cornwall only, was now extolled and exalted among kings and princes throughout the whole world.

² In margin: *De Sancto Mevenno, qui est in Britannia.*

³ *Sancti Mevenni.* Mevennus is the eponym of the important abbey of St.-Méen in the diocese of St.-Malo in Brittany, and of the parish of St. Mewan in Cornwall.

THE RELICS OF SAINT PETROC

But since we have digressed a little, let us return now to the task we have begun. This Martin, our brother in name only, because he was a sacrilegious wretch and possessed with a devil, was subject to melancholy and the spirit of *accidia*, and in consequence begged the Prior to [find him some occupation which would] give him relief. To cut a long story short, the Prior sent him to Devon, to our *villa* which we have there, named Newetona [*i.e.* Newton St. Petrock], to see that it was properly cultivated. But he behaved himself there maliciously and in a disorderly manner, so the Prior recalled him, and put him under discipline in the monastery. Furious at this, Martin sought diligently how he might avenge himself. Then the devil put into his mind the idea of committing a sin wicked above all others, namely to steal the body of our most holy patron Petroc, carry it away, and alienate it permanently, so that our house, thus destitute of its patron, might be entirely ruined. So he sought and found an opportunity, and took the aforesaid body out of the shrine, where he knew it had been previously placed by the hands of Bartholomew, Bishop of Exeter,⁴ and departed, and sought a distant land, carrying with him his sacrilegious plunder. Having escaped [from Cornwall], he crossed the Channel, and arrived at the Church of St.-Magloire,⁵ where he was received with more honour and kindness than he deserved. That same night there shone a light from heaven upon the ark (*arcam*)⁶ in which those relics were enclosed, some of those in the guest-house seeing it and marvelling at it, though as yet they knew nothing of the matter. So this sacrilegious wretch, condemned by his own conscience and fearing to be detected, early in the morning hired a horse to go on to Saint-Méen. He proceeded till he reached the ford called Cagarun,⁷ which they say can be

⁴ This seems to show that the relics had only recently been translated to a new shrine. The building of the priory church on a new site had been begun by Bishop Warlewast (d. 1138). It was probably only just finished, and the shrine of S. Petroc may well have been moved from the older church on the site of the present parish church shortly before 1177. This statement is therefore of great importance for the history of Bodmin priory. Martin stole the relics just after the Feast of the Epiphany (6 January) 1177.

⁵ *i.e.* the abbey of St.-Magloire at Léhon, a suburb of Dinan.

⁶ Presumably this means the place where the relics were resting, as, a few lines lower down, we are informed that they were carried in a sack.

⁷ The initial may be either a c or a d, the last syllable might be *rim* or *rmu*. As the road from Dinan to St.-Méen crosses the little river Garun, a tributary of the Meu, shortly before reaching St.-Méen, this lost place-name would seem to belong to a forgotten ford at that point. Such 'splashes', with a foot-bridge close by, are common both in Brittany and in Cornwall.

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crossed by any beast. The man from whom he had hired the horse he was riding bade him go on without hesitation, and he savagely whipped and spurred him till he drew blood, but in vain. Then the apostate, seeing that the horse refused to move, condemned by his conscience, suspected that this had happened by the will of God, on account of his irreverence towards the holy relics, which he was irreverently carrying behind him, and he dismounted immediately, unfastened the sack in which the relics were, and carried them over the foot-bridge close by; returning as quickly as possible to his horse, which he had meanwhile committed to his guide. Then he remounted, and passed the ford without difficulty, and thus, speaking little, but perchance thinking a great deal, he arrived at St.-Méen, where he was received at the guest-house [of the abbey], and returned the horse to its owner, paying him the charge due for its hire. The latter returned home, while he himself waited there till he should be summoned by the monks to dinner. When the hour for dinner arrived, he was called, with the other guests, to go and eat. So, being called, he hung his bag (*sacculum*) on to a certain beam in the guest-house, and committed it to the care of the boys whose duty it was to look after the said guest-house, and went in to eat, with the boy who had come with him on his journey. Meanwhile the boys just mentioned were regarding the bag with great interest, and touched it in turn with their hands. Feeling it, they found it contained bones, and wondered, saying, 'What can it be'? And one of them, more enterprising than the rest, approached the bag and partly unsewed it, and took out a rib, which he and his companions handled, amid general laughter, when, lo! their hands began to swell and their arms became rigid. Thus punished for their audacity, they ran and told the monks who had the charge of the outer court, showing them their hands and arms, and relating how they had suffered for touching those bones. The monks, divinely led to recognize something miraculous in what had happened, at once accosted their guest Martin, and questioned him closely about the object of his journey, asking what those bones were which he was carrying about with him, the touch of which had so injured the boys. Confused and terrified, he told them truthfully why he was travelling, and confessed that the bones were relics. The Lord Abbot was sent for, together with the chief personages of that Church, and before them he made a full statement, saying that it was the body of the most holy confessor Petroc which he had brought with him, and to remove all doubt he confirmed the same with an oath. After hearing this, the Lord Abbot straightway gave

THE RELICS OF SAINT PETROC

orders for a procession to be formed in festal array, and for the relics to be solemnly borne, with the song of hymns and canticles, from the guest-house where they were, to the church, where water sanctified by contact with them was kept, at the request of the people, to benefit the sick, by the grace of God and the merits of His Confessor.

But that most wicked of men, seeing himself everyway detected, proceeded at once to the Lord Rolland of Dinan (Dyneham), who was ruling over that country, and was Viscount [*i.e.* representative] of the Lord Geoffrey, son of the King of England [and] Count of Brittany, and, in the wickedness of his heart feigning simplicity, earnestly asked for an opportunity of speaking with him. Having obtained this opportunity, he sought to persuade him that he had brought with him to Brittany the chief of the Saints of Cornwall for the advantage and high honour of his lord the Count of Brittany, because Cornwall lawfully belonged to the same Count, and therefore he had, with great respect, handed over the chief of the saints of that country, viz. Patroc [*sic*], at St.-Méen, in the dominion of the said Count; vehemently asserting that, if the body of the most holy Confessor were prudently and diligently kept there, the whole of Cornwall would soon be subject to the County of Brittany [which belonged] to his lord the son of the King of England.⁸ Now the reason for his trying wickedly to make him believe this, was that if an attempt were made to bring back the relics, he [Rolland] might strive with all his power to retain them, out of fidelity to his lord the Count and for the benefit of the whole country. And he made this wicked suggestion, not that he cared at all for the benefit of that country, but because he was a sacrilegious robber, and wished by this cunning device to alienate those relics from our Church in perpetuity. But 'there is no wisdom, nor understanding nor counsel against the Lord' [Prov. XXI, 30]. Let us now return to the events which followed these things.

When therefore the fame of the mighty works and miracles which God worked for the honour of His holy confessor Petroc was spread abroad, there flocked thither the sick and the diseased, as usually happens in such cases, among whom were the boys whose hands and arms swelled and became rigid because they had before irreverently touched those relics—they were the first to come, praying and weeping, and asking with most earnest devotion that their hands and arms might be sprinkled with that holy water, confessing and firmly believing that

⁸ On what political situation the subtle intrigue suggested by Martin may have been based we do not know.

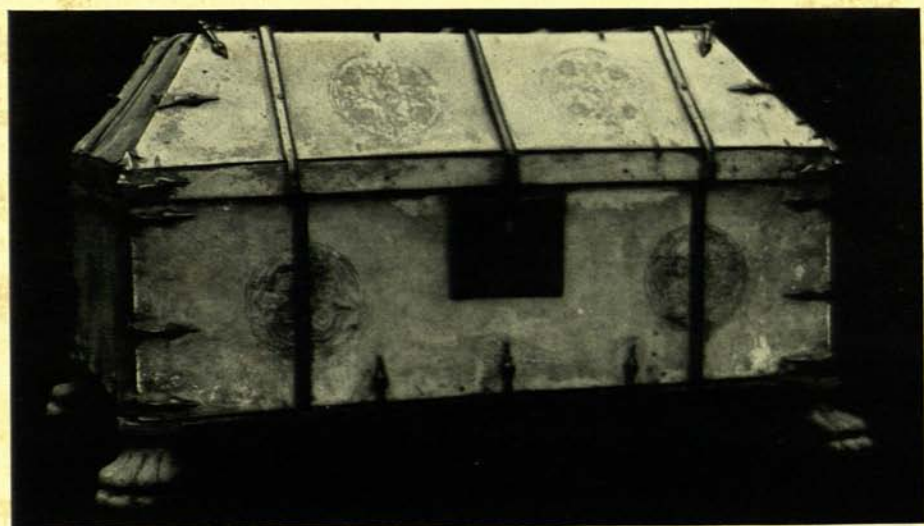
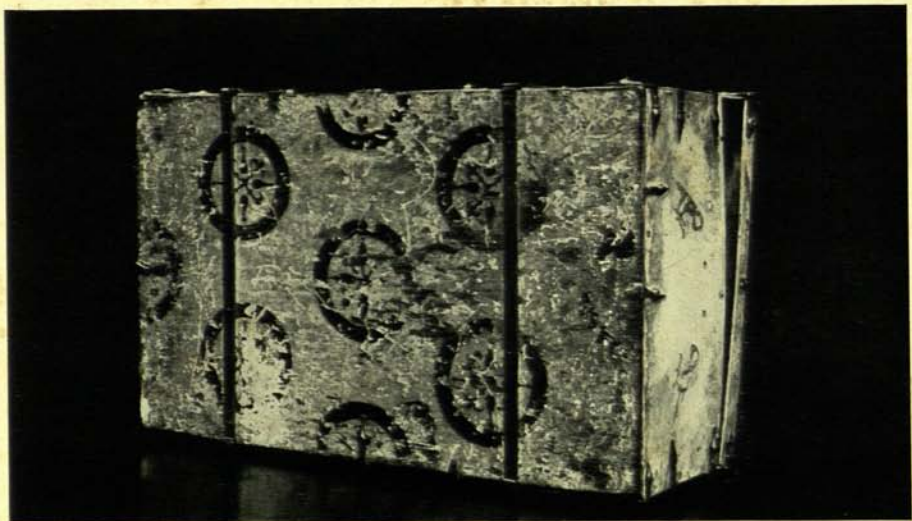
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in this way they could be cured, by the merits of the most holy confessor Petroc. So the monks, perceiving in the faith of those who made the request the efficacy of his petition, sprinkled them in the name of the Holy Trinity. And falling on their faces, and praying humbly before the relics, after a short interval they were completely cured.

Also a certain demoniac was brought one day by his relatives and friends, who, humbly and with great devotion, asked that in God's name they would give him some of that water to drink. And those who had the custody of the relics gave the holy water, in the faith of those who made the petition, to the demoniac to drink. After drinking it, he fell asleep, and when he awoke he was freed from the power of the demon, and gave praise to God.

Also a sick boy, who was able neither to live nor to expire, but suffered terribly in a prolonged agony, was brought to the place where the relics were, and his relatives asked that at least a drop of this water might be given him to drink, that so God, by the merits of His Confessor, might hasten whatever might be His will concerning him. And after tasting that sacred water, the boy's soul passed to the Lord. By these and other miracles the fame of this most holy body grew, till the news of its having been stolen from us reached, first, the Bishop of Exeter, and then us through him. So when we were informed of what had happened, we immediately sent our brother John, a monk of proved merit, on a voyage to Brittany, to find out in what province, and in whose jurisdiction, and in what church the body was detained, and for our Lord Prior, who was then at the Court of the King, we made a statement of the case by letter. Next, through our brother Adam, a monk of long standing and high character, we reported what had been done to the Lord Bishop, then at Exeter (although he knew it before we did, as we have said already), praying him in the Lord and beseeching him to make every endeavour to secure, by order of the Lord King and the assistance of the chief men of England, the return of these relics which had thus been stolen.

The greatness of the sacrilege committed against the body of the most holy confessor Petroc of Bodmin being now detected, and the fact of its removal to Brittany being made clear by many proofs, the Lord Bishop of Exeter, accompanied by his familiar friends, viz. Richard de Luci and Master Walter of Coutances, the King's *sigillarius*, afterwards Archbishop of Rouen, went humbly to the King, devoutly petitioning him, for the love of God and for the salvation of his soul and of the souls of his family, to send letters to Brittany, which



IVORY CASKET AT BODMIN

Believed to be the Reliquary in which the bones of Saint Petroc were brought back from St.-Méen, in Brittany

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was then under his suzerainty, and order these relics to be sent back. So the King, perceiving the humility and devotion of the bishop and of those who joined with him in the request, sympathizing with their grief, inclined his ear to their petition. Wherefore he immediately granted what the Bishop and his fellow-helps desired, and the King sent for the aforesaid Walter, his *sigillarius*, and commanded him to further the Prior of Bodmin's cause by writing to the prelates and chief men and officials of Brittany, commanding them that wherever in their country these relics were found they were to be returned without delay, adding that he was sending with the Prior his own messenger, who would confirm by word of mouth the order contained in the letter. So Master Walter, the King's *sigillarius*, rejoicing in what had been said unto him [Ps. cxxii, Vulg.], and at having received this mandate, departed at the first opportunity, and as he came out he met a certain one-armed man⁹ carrying an ivory casket which he was offering for sale. He felt that this was providential, and he bought the casket for the use of S. Petroc and ordered it to be taken with him, but considering it as rather fragile he took care to have another one made of wood, at his own expense, lining it with felt to protect the ivory casket, which he placed inside it. He then took council with the Bishop of Exeter, and the Prior of Bodmin and the other servants of the church of S. Petroc, as to the best way of drawing up the letter containing the King's orders concerning the relics, and directed the exact form of words they chose to be made out by the scribes. So the Prior, having received the King's letter, and those of his son the Count of Brittany, and of the Lord Richard, Archbishop of Canterbury, and of the Bishop of Exeter, and of the same Master Walter, the King's *sigillarius*, taking the casket thus prepared and the estimated expenses of the journey, accompanied by the King's messenger, having received the Bishop's blessing, set out to cross the Channel. After a calm passage with favourable winds he arrived at Coutances, where he was hospitably entertained at the expense of Master Walter. He now began to be worried and anxious about our brother John, who, as we have said already, had crossed the sea [before him] on this business. Now it came to pass that a certain boy who was travelling with the Prior happened to be in the market-place seeking for something he wanted, when, looking up, he saw that very brother John in the street searching for a lodging. As soon as he saw him he suspected (by God's indication)

⁹ or, 'mutilated man' (*emanco*, in French *manchot*).

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that he was the one about whom the Prior was so anxious, and he addressed him, asking what he wanted and where he came from, adding, 'Are you not the person who is seeking for the relics from Cornwall?' The other answered, 'I am, but I have no lodging, and am trying to find one'. 'Come with me', said the boy in a delighted voice, 'and I will bring you to your Prior'. There was great joy on his arrival, and they all glorified God for what had happened, feeling that it was His providence which had led to this fortunate meeting. Soon after this the Prior had an opportunity of speaking to a certain Canon of Coutances, the Lord Nicholas, a relative of Master Walter the Lord King's *sigillarius*, and talked to him about their journey. The Lord Nicholas courteously answered, 'I will myself accompany you on your journey, and will bring you speedily, if God will, by the merits of your holy father Petroc, to the Church of St.-Méén, where your relics are being kept'. Having made this arrangement, next morning they set out for St. Michael in Peril of the Sea, where they were received with honour and hospitably entertained. The Abbot,¹⁰ after reading the King's letter about the matter, sent one of his monks, an active man, named Robert, to journey with them in his place, as he himself was not enjoying his usual good health. They left [the Mount] immediately after lunch, and proceeded by easy stages, so as not to tire their company too much, visiting on their way the bishop-elect of Dol,¹¹ and other prelates and nobles to whom the King of England had addressed letters containing his commands concerning these relics, giving to each the royal missive intended for him, till at length they arrived at Dinan (Dinam), and enquired for the lord Rolland, who ruled over the [whole] country, and was the King's minister. Not finding him, they stayed there that night, and continuing their journey next day they passed by the village (castellum) of Léhon and spent the night at a certain hamlet (rus) called Caulnes (Calna). From thence they went on to the church of St.-Méén, alighting at an inn in the town, since the monks refused them hospitality.

¹⁰ The celebrated Robert of Torigny, Abbot of Mont St. Michel from 1154 to 1186. He was not often *debilis*: the greater part of the present abbey buildings were constructed by him.

¹¹ His name was Rolandus. Elected Bishop of Dol in 1177, he succeeded Bishop Johannes, who died 27 January that year. He was not consecrated till 1184, which seems to have been (as we have already suggested) the date of the composition of Robert of Tauton's work.

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Now when the Prior and his party entered the church to pray, a globe of fire in the shape of a helmet descended upon the church, in the sight of all who were in the market-place. But the Prior perceived that his presence and that of his party was viewed with suspicion by the monks, and, after conferring with the others, they all returned to Dinan the very same day. From thence the monk Dom Robert and Nicholas the Canon of Coutances returned home. It was the vigil of Pentecost. The others, after their return to Dinan, stayed there quietly until they were able to communicate with the Lord Rolland by sending to the place where he had gone. Rolland meanwhile had heard about the whole affair from a messenger whom he had sent into England to the Lord King on his own business, and who had just returned, and learning that the Prior of Bodmin was already come to Dinan with the Lord King's commands to him, he summoned Geoffrey of Montfort (*Gaufrido de Munfordia*), who was the *advocatus et dominus* of the monks of St. Méen, and consulted with him as to the best way of quietly settling the matter. Meanwhile the Prior of Bodmin, hearing that Rolland and Geoffrey of Montfort were at St.-Méen, for he had sent for him [*text imperfect*] they took with them the Abbot of Léhon, a learned and prudent man, and joined them there, and, as soon as they had opportunity, presented the orders of the Lord King. Rolland and Geoffrey, having listened to their story, and seen exactly how the matter stood, received them with honour and entertained them courteously. Then they sent them, accompanied by the Abbot of Léhon and other capable persons, to the Abbot of St.-Méen and his monks, with the letter from the Lord King, ordering them peremptorily to hand over those relics to the Prior of Bodmin and his brethren. To cut a long story short, the monks, though with great reluctance, after making many attempts at evasion and putting every obstacle in the way, at last placed the body of the most holy confessor our father Petroc, upon their high altar, and there, in the presence of the venerable Abbot of St.-Magloire and of the barons, viz. Rolland and Geoffrey of Montfort, and of many others who had come there for that purpose, permitted them to take it, the Abbot and the sacristan of the same church placing their hands on the holy gospels and swearing that they were the same relics, whole and entire, which the sacrilegious Martin had brought there. The Prior and his brethren approached, and, carefully examining them, recognized them by many signs [as being the genuine relics], and had no further doubts about the matter. Opening their ivory casket, they found that, by God's dispensation, it happened to be marvellously adapted for

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receiving those holy bones, being just the right size, neither too big nor too small. After they had been placed therein, the Lord Rolland of Dinan approached, and with the greatest reverence handed them over thus enclosed to the Prior of Bodmin, with a letter, to be taken to the King of England at his own expense. Then, in testimony to future generations of what had happened, he sent his own letters patent to the church of Bodmin to be preserved there.

So the monks of St.-Méén, seeing themselves totally deprived of these holy relics, inwardly touched with compunction, confessed with tears that the Lord had, by the merits of this His Confessor, worked xiv undeniable miracles in their sight since these relics had been solemnly received in their church. And when the relics had been laid together, and, with the greatest respect, enclosed in the shrine, as has been said, the Prior of Bodmin and his party, their spirits refreshed with holy joy, arranged the same day with the barons aforesaid to begin the return journey. Not to linger over details, suffice it to say that they went forward day by day, bearing the ark of their lord in triumph, in many places received with great ceremony, amid processions both of monks and of clergy, and good success everywhere attending them. They sailed for England, and having arrived at the city of Winchester, after two days they met Master Walter, their counsellor and protector and the King's *sigillarius*, who received them courteously and gave them the kiss of peace, and, rejoicing in their success, showed them every mark of kindness. Later, when the King had come, they sought for an opportunity of speaking with him, and this having been, by God's grace, obtained, and a suitable place and time chosen (thanks to the good offices of Master Walter), they came into the presence of the Lord King with their relics. They handed him the letter of the Lord Rolland of Dinan and of the other notables of Brittany to read, as evidence of what had taken place. After he had read it, the king, in the presence of his Court, which was unusually numerous, as a council of prelates and nobles had been summoned, ordered the casket to be opened. This was done, and when the Lord Bartholomew, Bishop of Exeter, had opened it, they saw that treasure, 'more precious than gold and the topaz' [Ps. cxix, 127, Vulg.]. At the sight thereof the King, and all his court with him, humbly prostrated themselves and venerated the body of the most holy confessor Petroc. After a short interval, he received in all simplicity at the hands of the Bishop of Exeter a benediction with the head of Saint Petroc, and after gazing with great reverence at all those sacred bones, he retained for himself three joints,

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first humbly asking permission to do so from the Bishop of Exeter and the Prior of Bodmin, and also a rib, which he enclosed with fitting honour in a silver case, and sent it to St.-Méen.¹² Then, after prayer, he closed the casket with great devotion, and offered a valuable silk pall, cunningly embroidered, to shroud the relics. After this, the Prior of Bodmin and his party, who were in charge of the relics, on the first opportunity, after obtaining permission from the Archbishop of Canterbury and the other bishops and having received their blessing, set out to return home and advanced by easy stages to Exeter, where they were received with great respect in the Church of the Episcopal See, and committed the relics to the protection of God and to the care of the secretary¹³ of the same Church, waiting till it should be convenient for the Lord Bishop to honour the ceremony of their solemn return home with his presence, as he had intended. As soon as he was free, the Lord Bishop came, and a procession was formed in festal array, all the inhabitants of the city joining in it, and the Bishop led the relics forth outside the city with hymns and spiritual songs. After a short sermon to the people there he gave them his blessing, and the people returned to their city. Meanwhile the Bishop continued his journey to Cornwall, accompanying the relics, and arrived at the church of Launceston [Lanstanatuna], where the Prior of the same church¹⁴ and his brethren came in festal procession to meet the relics with due honour, as their membership of the Order [of Saint Augustine]¹⁵ required, and received them with joy and exultation, and kept vigil before them all night [in their church] with prayer and great devotion. In the morning a procession was formed, in which the people [of the town] joined, and the Prior of Launceston and his brethren escorted the relics and their bearers with great ceremony, as was meet. And when they had come to a certain place [outside Launceston] which had been fixed on beforehand, they received the Bishop's blessing, and returned to their church, and the Lord Bishop and those who were with him continued their journey to Bodmin. As they approached, the Convent of Bodmin went forth to meet them in a procession as magnificent

¹² Documents of the 17th and 18th centuries relating to the Abbey of St.-Méen show that the Abbey then claimed to possess St. Petroc's head.

¹³ *sic*, a slip for 'Sacristan'.

¹⁴ The priory of St. Stephen's at Launceston.

¹⁵ Both Bodmin and Launceston Priors belonged to the Order of Regular Canons of S. Augustine.

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as they could possibly make it, and with them the chief personages (*optimates*) of almost the whole of Cornwall, both men and women, accompanied by a crowd of people of both sexes which could not be numbered. And when they drew near to the church, the Lord Bishop made a station, and preached to the people, briefly informing them of all that had happened, and promising to exhibit the relics next day, that all might see them, and so they carried them into the church that day with a joy and honour that cannot be described, and placed them with great reverence on the high altar. The night was passed in rejoicing and spiritual gladness for the coming of so great a father. Next morning, after divine service, the people having come together, the Lord Bishop proceeded to do what he had promised the day before. First, he took out of the casket, with the greatest attention and respect, the head of the most holy Confessor, and then the other bones, and showed them openly to all the people. Afterwards he ordered to be read in their hearing the King's letters patent, and those of the Lord Rolland of Dinan, and his own, and those of the notables of Brittany, witnessing that there was no fraud, nor any diminution of the relics. He proceeded to give the people a few words of exhortation, and then he divided the relics, as he had previously determined, placing the head, with the bones nearest the head, and certain fragments, in the ivory casket, while all the other bones he caused to be enclosed in the great gilded shrine, in his presence and in the presence of all the congregation. Silence was then called for, and the Bishop relaxed 40 days of that year's penance to all who had come together in honour of this most holy Confessor, or should do so down to the Feast of All Saints. And in future, on the day when this solemnity is celebrated, and for eight days after,¹⁶ he granted for ever an indulgence of 20 days from the penance enjoined on them, to all who should piously and with devotion come to this solemnity. All this was done in the year of our Lord's Incarnation 1177, in the month of September, on the 15th day of the month, in the reign of Henry, son of the Empress Matilda, and in that of his son, also called Henry, who had been crowned King a few years before, Richard being Archbishop of Canterbury,

¹⁶ This explains why William of Worcester, when he visited Cornwall in 1478, found in the kalendar of Bodmin priory, on 14 September, the words 'Exaltacio Sancti Petroci' (he adds 'die exaltacionis Sanctae Crucis'). The anniversary of the return of the relics from Brittany was clearly observed as a festival of the first-class, with octave, down to the Reformation.

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Bartholomew being Bishop of Exeter, to the honour of Our Lord Jesus Christ, Who with the Father and the Son [*sic*] and the Holy Ghost liveth and reigneth for ever and ever. Amen.

NOTE

Sir John Maclean, *History of the Deanery of Trigg Minor*, I, 231, says, 'There is belonging to the corporation of Bodmin, and now in the custody of the town clerk, a very curious and ancient ivory casket. According to tradition it is the same . . . used in bringing back the relics of St. Petrock'. It is 'in length about 1 ft. 6 ins., about 1 ft. in breadth, and in height about 10 ins.'. Mr Alexander Nesbitt, F.S.A., pronounced it to be 'an example of oriental work of about the 12th or 13th centuries'. It is said to have been hidden in the room over the porch of the parish church, and accidentally discovered in the 18th century, but Maclean does not mention this.

[The casket was exhibited before the Society of Antiquaries on 2 February 1871, and reported in *Proceedings*, Series 2, v, 87. There it is stated that few of such coffers have survived, and that there is difficulty in determining their place of manufacture. It is thought they were of the Hispano-Moresque School. Particulars of the casket are also included in L. Jewitt and W. H. St. John Hope's *Corporation Plate*, 1895, I, 76-8.—EDITOR].

Kilwa: a Review

by NELSON GLUECK

TRANSJORDANIEN: VORGESCHICHTLICHE FORSCHUNGEN, edited by Hans Rhotert, with contributions by Prof. Franz M. Th. Böhl and Dr K. Willman. pp. xii, 251, 63 photographs, 294 line-drawings and 3 maps. Verlag Strecker und Schröder, Stuttgart, 1938.

DURING December 1932, a joint expedition of the Transjordan Department of Antiquities, and the American School of Oriental Research, Jerusalem, under the direction of Mr and Mrs George Horsfield and the reviewer, undertook a trip of archaeological reconnaissance through the desert of eastern Transjordan from Mafrak to Kilwa. The existence of ruins at Kilwa was known to us, and they formed the ultimate objective of our journey. Gertrude Bell seems to have been the first European to have visited these ruins on her way to Hayil in 1914, merely recording the fact in her *Letters*. We were able to establish the fact that the buildings there belonged to a Christian community of c. A.D. 1000. They housed a monastic settlement, marked also by numerous small cells in the vicinity of the main buildings, which existed in the midst of a Moslem world. The early Christian settlement at Kilwa was well situated. It was on the north-south track leading from Bayir Wells to Kilwa; the Egyptian west-east Hajj route from 'Aqabah crosses the Jebel Tubaiq near Kilwa, where there were once some wells, and also a small reservoir. Before the advent of Mohammed, orthodox Christian hermitages lay on the main caravan routes. The monks went to Arab fairs, where they helped nomad travellers with water and tended the sick. The Christian inhabitants of Kilwa must be thought of as functioning in similar capacities. The main group of Christian buildings at Kilwa, constructed of basalt blocks, resembles in general type the buildings in the southern Hauran. Several hundred metres northeast of the Christian site we noticed several constructions, at the base of a small hill of Nubian sandstone, which turned out to be lime-kilns, apparently contemporary with the monastic settlement. A further examination of the hill however, revealed to our amazement that every smooth surface on it was covered with prehistoric rock-drawings, some of them superimposed upon one another. Gertrude Bell had missed them, but they

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must have been familiar to the early Christian inhabitants of the site. We were the first Westerners to see the rock-drawings, which testified to the presence of prehistoric man in this remote corner of the north Arabian desert. Unfortunately, we were not equipped for a long stay at the site. We had furthermore spent most of the day examining and planning the buildings of the Christian settlement. Mrs Horsfield and the writer snapped pictures right and left, during the short time that remained at our disposal. We had no time to look for stone implements, and not even enough time to be certain that we had found all the visible rock-drawings, nor to remove the sand from crevices where others were bound to be found ; much less to undertake a thorough investigation of the surrounding district, or to see whether or not any other rock-drawings could be found. All too soon we were compelled to leave our work and dash through the treacherous sands to the previously prepared camp. We did not return at a later date to Kilwa, but rested content in publishing as much as we had seen of Kilwa, and our conclusions with regard to it.¹ None of us were prehistorians, and we felt that a second expedition to the site should be undertaken by a more competent group with specialized training and equipment. It was not till 1934 that such a group went to Kilwa, under the general direction of Leo Frobenius, and led by Hans Rhotert. It remained there for two months during part of November, December 1934, and part of January 1935, and made an exhaustive study of the prehistoric rock-drawings, and of the entire district surrounding Kilwa. The book under review gives the full report of the results of this expedition.

It is a well got-up book, with its beautiful photographs, excellent line drawings, and spacious margins. But it is a pity the author could not have refrained from intruding political propaganda into a first class scientific work. The following statement, translated from the introduction, p. 23, is as incorrect in its facts as it is malicious in its innuendos : ' The inhabitants of Transjordan, threatened in their economic life by the Jews and dominated politically by the English, see in the German developments since 1933 a struggle for freedom which arouses their admiration—a struggle which may lie in store for them, or which they may not outlive. How often were we asked en route about the " Pascha kebir, kebir, kebir ", the very great Ruler, by which naturally

¹ G. and A. Horsfield and Nelson Glueck : ' Prehistoric Rock-Drawings in Transjordan ', *American Journal of Archaeology*, 1933, xxxvii, 381-386, 529, plates xxxix-xlv; Nelson Glueck, ' Christian Kilwa ', *Journal of the Palestine Oriental Society*, 1936, xvi, 9-16.

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the Führer of the German people was meant'. Cannot the price for handsome publications be raised in present-day Germany without sycophantic genuflections? There are no Jews in Transjordan. Its economic life, it is true, is in difficult straits now partly because of the political disturbances in Palestine, and because even in peaceful times Transjordan could only benefit indirectly from the upswing which has marked the economic life of Palestine in the post-war period. The English have brought a peace and possibility of development to Transjordan which it has not known for centuries. The reviewer, who has travelled into almost every nook of Transjordan in the company of Arabs only, has yet to hear any complaints against the British mandatory rule as such, although naturally there are some local grievances. The population of Transjordan, apart from its general appreciation of the good government it is now enjoying, remembers too vividly the days of the previous government to have even the faintest hankering for a 'struggle for freedom', to be dominated perhaps by a Germany which has never given up the dream of Berlin to Baghdad?. As for the numerous times when Rhotert's party was greeted with the equivalent of Heils to the 'great, great, great Pasha', the readers of this review may be reminded of the fact that the 250 kilometres between Maan and Kilwa, the distance most frequently traversed at 50 kilometres an hour by various members of the Rhotert party, is a stony or sandy desert, where rarely a lonely Bedu may be seen, or through which an occasional herd of camels is driven. Questions were undoubtedly asked occasionally about the well-being of the 'great Pasha'; but to any one even slightly acquainted with Transjordan, it is well known that when el-Pasha el-Kebir was referred to, it was the extremely capable, and much loved Peake Pasha, Col. F. J. Peake, formerly Officer Commanding the Arab Legion in Transjordan, who was meant. He worked with Lawrence during the war, and, together with a handful of other outstanding Englishmen, under the leadership of the astute and loyal Emir Abdullah, remained when peace was declared, to organize and administer the country.*

It has been necessary to refute the incorrect statements in what may be called the impressionistic part of the book, particularly because they have not even the *Schönheitsfehler* of being honest mistakes in scientific judgment, and because they do serious injustice to the splendid

* The insertion of irrelevant political comments in a scientific publication is rightly criticized. Such comments merely serve to create ill-feeling, and thus prejudice future expeditions.—EDITORS.

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subject of Transjordan, which is also the title of the book, and which is as such totally misleading, except for the sub-title of 'Prehistoric Studies' on the inside title-page. To judge from the cover-title, one might expect a many-faceted book about Transjordan, its ancient past, its involved present, its inhabitants in the various periods of its history, its climate, geography, geology, topography, and other aspects of its complex whole. Instead, there is the account in the first chapter of the book of the equipment and journey of the expedition by car to Istanbul (not Constantinople !), the description of customs difficulties there, the journey by boat to Haifa, and a short stay in Jerusalem. The description of the journey to Amman, Maan, and Kilwa, is rattled off in a few pages. The largest part of the book concerns itself most properly with Kilwa ; but Kilwa is a single, isolated, and extraordinary part of Transjordan, and is only a short distance from Saudi Arabia. Kilwa is the only place of its kind in all the Near East, one may say the only place of its kind east of Europe and northeast of Africa ; but it can certainly not be representative of all Transjordan. Indeed, the reviewer fails to understand why the book was not honestly called 'Kilwa', after the tremendously important subject-matter with which it is chiefly concerned. (It is an unconscious reflection on a fine desert bird, due no doubt to a printer's error, p. 47, to call a desert-bustard a 'Turkey Bastard'). The author of the book need not take umbrage at these strictures, because in his introduction he observes that the scientist will be interested especially in the third and fourth chapters of the book. We agree ! The foreword, introduction, chapter 1, and almost all of chapters 2 and 5—the latter two chapters being diary excerpts—may well be skipped. Having in these sections of the book devoted so much space to ordinary travel accounts and photographs, we find it regrettable that Rhotert did not give more attention to the buildings of Christian Kilwa than he did. Together with his other excellent photographs, he might well have included photographs of the interesting lintel over a monk's cell at Kilwa, on which a Byzantine cross and an Arabic inscription are incised, as well as another Arabic inscription found in one of the main buildings.²

The third and fourth chapters of the book, which form its major part, are of primary scientific importance. Whether or not one agrees with Rhotert in all of his presentation, it is a pleasure to be confronted with a detailed, meticulous, scientifically objective account of the

² Cf. *Journal of the Palestine Oriental Society*, 1936, XVI, pl. 1, and pp. 12-13, where Prof. L. A. Mayer of the Hebrew University discusses these inscriptions.

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prehistoric finds at Kilwa, fully documented and copiously illustrated. Every stone implement discussed is illustrated with either a line drawing or a photograph, or both, and the illustrations of the rock-drawings leave nothing to be desired. The line drawings of the siliceous (quartzitic) sandstone implements made by Miss Elizabeth Krebs are especially worthy of praise. A map of the Kilwa district is appended to the book, enabling the reader easily to locate the places where finds were made. This map is supplemented by an excellent panorama photograph of the Kilwa basin made by Rhotert, despite the lighting difficulties encountered in the district. At the beginning of the book are two maps; on the first are traced the various trips made by the Deutsche Inner-afrikanische Forschungs-Expedition, under whose auspices also the Kilwa expedition was undertaken, during 1934-35. The second is a map of Palestine and Transjordan, giving among other places all the points visited by the expedition.

The lodestone which attracted the Rhotert expedition to Kilwa was the prehistoric rock-drawings which had first aroused our enthusiasm. The hill on which the rock-drawings were found has been named the 'Horsfieldberg' by the German expedition. One of the most beautiful of the rock-drawings is that of an ibex. It is about 50 centimetres high, and is faithfully and artistically rendered. The burin marks are clearly visible, the lower left end of each stroke being slightly deeper than the upper end. Some of the lines were obtained by chiselling from either edge of the desired width of the line, the deepest indentation being in the middle. The artist possessed considerable ability. The full gracefulness and beauty of the delicate animal, which is still to be found in the Transjordan desert, have been caught and imprisoned in the lines on the stone. Details are finely represented. Nostrils and neckline, horns rising and sweeping back gracefully from the head, then curving and tapering to sharp points touching the back, the foreleg lifted in motion—all these features bound together in a delicate yet strongly portrayed whole—give the rock-drawing a vibrant reality. The head of the ibex is raised, and from its mouth stream two lines, possibly representing blood, as Abbé Breuil suggested to the reviewer in 1932. It seems to represent a picture of a wounded ibex poised in flight. Rhotert agrees that the two lines extending downward from the mouth of the ibex represent streams of blood, but suggests that this and other related rock-drawings represent sacrificial scenes. The reviewer confesses having completely failed to see, when he was in Kilwa, the crude drawing of a small human figure with outstretched arms, standing on the

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back of this ibex, which is quite clear in the excellent photograph, pl. 10 bottom, published by Rhotert. Rhotert is uncertain as to whether or not this rock-drawing is contemporary with that of the ibex. For us there is no question, but that it is much later. The lines are much shallower, the workmanship quite different, and the whole drawing so obscure, that it escaped our attention in the light in which we saw it. Indeed, we should be inclined to assign it to a period as late as the Thamudic.

One of the most interesting of the rock-drawings, and the only one of its kind on the Horsfield-hill, is an elongated, narrow-headed, horned animal, presumably an ox, over two metres long, superimposed over several smaller and previously incised drawings of ibex. Below the left foreleg of the ox is another small, considerably weathered rock-drawing, also probably of an ibex, and earlier than the drawing of the ox. Its only depicted hoof is attached in reverse fashion to its hind leg by a single line. Rhotert, however, considers it a hitching line. Squatting under the ox, with arms raised above his head, is the highly stylized figure of a human being, resembling one from the Fezzan rock-drawings in North Africa. In his right hand he is holding, according to Abbé Breuil, to whom we showed a photograph of this rock-drawing in 1932, what may be a spear, which he has thrust into the side of the ox. Abbé Breuil suggested to us at the time that this composition represents a hunting scene. There is no question but that the ox and the human figure squatting under it are contemporary. Rhotert thinks that the spear may merely be the end line of the back side of an ibex, over which the drawing of the ox has been superimposed. We believe, however, that the deepness of this line is more in harmony with the lines of the ox and of the man than of the ibex, as can be best seen perhaps in the photograph, pl. XL, 1, which we published in the *American Journal of Archaeology*. We agree, however, with Rhotert, that these drawings do not represent an ordinary hunting scene. Indeed, it seems more strange to the reviewer than ever, that if indeed this is an ox, and not some particularly crude representation of some animal more characteristic of the desert, that it should be portrayed at Kilwa at all. We confess also being sceptical as to whether or not the rock-drawing on p. 212 bottom, and pl. 23 bottom, represents a plough, as Rhotert would have it. We missed seeing this drawing, in addition to many others which the Rhotert expedition discovered on Horsfield-hill ; but to our mind, the photograph on pl. 23 bottom, gives the impression of an animal much more than of a plough. Of much interest is the rock-drawing (p. 208, pl. 22), discovered by the Rhotert expedition, representing a

human pair in seated position, clasped together in close embrace. It is very similar to the limestone statuette found by Neuville in the Wadi Khreiton near Bethlehem, which Neuville assigned to the Natufian period. Rhotert assigns this rock-drawing to the same period. There is another rock-drawing at Kilwa, which we had previously seen, but not published (cf. pp. 183, 212, pl. 23 top), which Rhotert suggests may represent a male figure, enclosed within a double circle. We believe he has misread the confusion of rock-drawings contained in this group. What he considers to be the lines of a double circle are obviously the lines of ibex horns, such as are found on many of the earliest of the Kilwa rock-drawings, and they are all that is left of the rock-drawing of the ibex, which originally covered this particular surface of rock. If my memory serves me correctly, this group of drawings is to be found on the flat surface of a small flat shelf or space of the Horsfield-hill. Inasmuch as this space was particularly accessible, it was bound to be utilized for more or less artistic efforts during various periods. What Rhotert takes to be the outlines of a male figure, are actually, it seems to us, the outlines of two very crude little animals, the one near the bottom of the excellent photograph being particularly clear. It is of a type which undoubtedly belongs to a comparatively late historic period.

Whether or not one agrees with Rhotert's datings for the prehistoric rock-drawings at Kilwa, or with his interpretations of them, he and his capable staff have furnished photographs and drawings of such general excellence that the competent reader is placed in a position to judge for himself. On the basis of the photographs the reviewer showed to Abbé Breuil and Professor Dorothy Garrod in 1932, these two authorities then dated most of the prehistoric rock-drawings that we had discovered to the Palaeolithic and Neolithic periods. Naturally, they might have changed their minds had they actually seen the rock-drawings *in situ*. As the result of his careful study of the prehistoric rock-drawings, Rhotert has come to the conclusion that 'an Upper Palaeolithic dating for the rock-drawings is out of the question'. He would date the earliest of the rock-drawings to the Mesolithic period, beginning with Natufian 1, and carry their development down to the Bronze Age. He assigns most of the drawings of ibex to the Mesolithic period, and suggests that perhaps the drawing of the human pair locked in embrace is also to be assigned to this period. He thinks furthermore that the drawing of the ox, with the figure of the squatting human beneath it, may belong to the latter part of this period. Rhotert is probably correct in assigning the 'leopard', which he would classify as a kind of

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lizard, not to the Palaeolithic period, as we had previously done on the authority of Abbé Breuil, but to a period considerably later than the Mesolithic. To judge from the Thamudic inscriptions and the contemporary human figure super-imposed over it, which it thus antedates, it is, however, to be dated somewhere in the pre-Christian era.

When Rhotert speaks of the Bronze Age, both with regard to implements and rock-drawings, he cannot now mean what he says. He equates Bronze Age with Ghassulian, following the date for Ghassulian given at first by Mallon and Köppel. However, from the time of the very first reports dealing with the results of the excavations at Teleilat Ghassul, Albright has maintained that the Ghassulian Culture must antedate the Early Bronze Age, since its pottery is so radically different from Early Bronze Age pottery, especially in its lack of decorated ledge-handles. At first, Albright dated the Ghassulian culture to the second half of the fourth millennium B.C. ; more recently, he has pushed it back into the first half, with its beginning perhaps in the latter part of the fifth millennium B.C. Proof of the general correctness of Albright's position has now been furnished by the latest excavations at Jericho. In 1935 and 1936, Garstang excavated in the lower levels at Jericho, and found pottery with Ghassulian affinities below a thick stratum of typical Early Bronze Age wares, thus proving their respective positions. Other excavations in Palestine have furnished additional corroboration of the fact that the Ghassulian or Chalcolithic period precedes the Early Bronze Age, and cannot be equated with it. This relative dating has now been accepted by Father Köppel himself. It may also be added that the relative sequence of cultures immediately preceding and following the Ghassulian, as now revealed by the Jericho excavations, is : Natufian, Tahurian, Ghassulian, and Canaanite. Wherever, therefore, Rhotert uses ' Bronze Age ', the reader of his book may substitute ' Ghassulian ' or ' Chalcolithic '.³

In addition to the prehistoric rock-drawings, Rhotert describes in detail and furnishes excellent photographs and many fine drawings of the much later and much less skilful rock-drawings also to be found at Kilwa, most of them on Horsfield-hill. He also discusses the related rock-drawings in the Wadi Rum and at el-Quweirah. Many of the late rock-drawings at Kilwa were found associated with Thamudic inscriptions, and are probably contemporary with them. In a brief

³ For the most recent and correct summary of the discussion about the Ghassulian culture, cf. G. Ernest Wright : *The Pottery of Palestine from the Earliest Times to the End of the Early Bronze Age*, pp. 14-41 (reviewed *ANTIQUITY*, March 1939, pp. 126-8).

separate chapter, Franz M. Th. Böhl deals with the inscriptional material recorded by the Rhotert expedition, and points out, in agreement with us, that none of the inscriptions precede the Christian era, and most of them may be regarded as Thamudic and assigned to the third century A.D. Böhl points out furthermore, that there are definitely no proto-Sinaitic characters among the Kilwa inscriptions.

In addition to its exhaustive study of the rock-drawings, the Rhotert expedition made a detailed study of the district surrounding the Horsfield-hill, and collected over 5000 stone implements from some 27 different sites in this area, as well as some scattered finds. A graphic table of the finds classified both according to place and period is given. The finds on site 19 are particularly interesting, because this site is immediately in front of Horsfield-hill with its prehistoric rock-drawings. Rhotert carefully considers the possibility of a connexion between the date of the implements and its bearing upon the date of the prehistoric rock-drawings. He is correctly cautious in drawing conclusions from the juxtaposition, but finds a relationship between the rock-drawings and the implements of site 19 which seems to be quite reasonable. After a careful examination of the implements on site 19, Rhotert concluded that they are mainly Natufian, *i.e.* Mesolithic, and extend down to the Early Bronze Age (read Chalcolithic), although some of them may have originated in the pre-Mesolithic, *i.e.* in the Middle Aurignacian period. Having dated the earliest rock-drawings on the basis of the known development of prehistoric art to the Mesolithic period, we feel that Rhotert is quite justified in adducing additional support for this dating from the fact that the only implements which can be directly connected with the Horsfield-hill belong predominantly to the same period. On some other sites, the expedition found implements belonging to the Lower and Upper Palaeolithic periods. A special group of implements, which seems to be peculiar to Kilwa, has been designated as belonging to a 'Kilwa Culture', and has been assigned to the Mesolithic period or later. The care with which Rhotert has dealt with his materials is indicated also by the fact that he includes in his report drawings and descriptions of implements which he cannot classify. In addition to Böhl's report on the Kilwa inscriptions, there is also an interesting discussion by K. Willmann on the geology and petrography of Kilwa. The scientific sections of the book under review form a valuable contribution to the rapidly growing mass of information about the prehistory of Palestine and Transjordan.

The Volcanic Destruction of Minoan Crete

by SP. MARINATOS

Director of the Greek Archaeological Service

IN the long history of Minoan civilization two great catastrophes are discernible, of which the famous Cretan palaces themselves provide the chief source of our knowledge. Everywhere the catastrophes are seen to be contemporaneous. We can distinguish a period of the first palaces (MM) and a subsequent period of the second palaces (LM). There is no perceptible break in the development of the civilization as a result of these catastrophes. For this reason, the theories that the palaces were overthrown by invaders from abroad aroused opposition from the first. Usually the Achaeans—and even the Hyksos—were suggested as the destroyers.¹ By this theory, however, it was not possible to explain two facts: the decorative arts continue on their way undisturbed, and the second palaces are built at once on the ruins of the first and are still unfortified. The Cretans would not have been so foolish as gratuitously to provide easy loot for fresh invaders.

After the great earthquake of 26 June 1926, which did such damage to the Candia Museum, Sir Arthur Evans put forward a new theory, suggesting that the catastrophes were caused by earthquakes, and this is much nearer to the truth.² There can be no doubt that at Knossos, lying in the district of Crete which is most susceptible to earthquakes, the destruction of the palace on many occasions was due to them. The attempts made in recent years to tabulate the occurrences of earthquakes show us that in every hundred years the Herakleion district experiences at least two severe, destructive ones.

When, however, we attempt to explain the various catastrophes throughout the island by common earthquakes, the problem begins to present difficulties. The other parts of Crete are less susceptible³; many districts, indeed, are almost wholly immune. Moreover, earthquakes are usually confined to a very small area. So far, even in the

¹ Edouard Meyer, *Geschichte des Altertums*, 22, 1, pp. 43-4.

² *Palace of Minos*, II, 313 ff.

³ *op. cit.* II, 319.

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Herakleion district which suffers so much, each earthquake has, as a rule, destroyed only a few villages and the surrounding districts have been little affected. It is, therefore, worth our while to examine in detail the two great catastrophes which befell the Cretan palaces.

Three palaces are already known—at Knossos, Phaistos and Malia. It is very probable that we may discover one, perhaps two, more. At the eastern end of the Mesara, under the shadow of Mount Dikte, where the rich cave of Arkalokhori was excavated a few years ago, we can confidently expect the discovery of a new palace.⁴ In the eastern districts of Crete, Mirabello and Siteia, a brilliant school of art with distinct local characteristics flourished from the beginning to the end of Minoan times. It is probably there where we must look for the home of the magnificent 'marine style'.

Until, however, new excavations give us fresh light, we must base our conclusions on the three palaces already known. Recent discoveries have shown that at Malia, as at Knossos and Phaistos, we have two periods of palace construction. It is not yet absolutely certain when the first palace at Malia was destroyed, and though the beginning of MM II is suggested⁵ as the date, M. Chapouthier now believes (as he kindly tells me in a letter) that it took place in the MM IIIa period. The two other palaces—at Knossos and Phaistos—were destroyed during the splendid 'first acme' of Cretan art, the 'Kamares style' period. According to Sir Arthur Evans a simultaneous 'seismic' catastrophe befell Knossos and Phaistos during MM IIb, about 1750 B.C.⁶

If, meanwhile, we look at the great store-jars which were buried in the store-rooms of the two earlier palaces, great doubts arise in our minds. Was the destruction of Phaistos absolutely coincident with that of Knossos? This would be essential if one and the same earthquake were supposed to cause both. The store-jars at Phaistos are, as a rule, smaller and more archaic-looking than the corresponding Knossos

⁴ For Arkalokhori see the preliminary reports in *Arch. Anzeiger*, 1934, 251 ff, and 1935, 248 ff.

⁵ *Mallia* (the correct orthography is Malia), II me. Rapport (Études Crétoises, IV), p. 50.

⁶ *Palace of Minos*, II, 319 and cf. I, 258, 299, 315-16. Pernier (*Festós*, pp. 452-3), like N. Åberg, dates the destruction at the end of the Middle Minoan Period, i.e., about 1700 B.C., but it is noteworthy that Pernier seems not to distinguish the two separate catastrophes at Knossos, that of MM IIb and that of MM IIIb. We conclude from his passage on page 453 that he considers that the new palace at Phaistos was built during the advanced MM III period.

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examples. The 'knobbed' store-jars are more squat and have fat, swollen bodies. They seem earlier when contrasted with the developed, tall appearance of those at Knossos.⁷ It seems, therefore, as if the destruction of the Phaistos palace took place during MM II, but before that of the Knossos palace. In any case, it appears certain that the first palaces on the Cretan sites were not destroyed simultaneously. The palace at Knossos cannot have been destroyed by anything but an earthquake, but, in the case of the other two sites, we cannot exclude the possibility that they were destroyed through violence by enemies, who in fact sacked both. The destroyers cannot have come from abroad. It was clearly the dynasty of Knossos which must have imposed its power by violence. The Knossos palace is rebuilt on more grandiose lines than the two others, with wide spaces and great vistas. Its huge store-rooms and other riches, the progress of its complicated, bureaucratic system of writing and, in general, the great prosperity of the Minoan city with its magnificent buildings surrounding the palace, show clearly that here was the centre of power. Knossos had no rich and fertile territory round about, such as the Mesara plain which surrounded Phaistos. It is, therefore, clear that the vast store-rooms of the Knossos palace received the tribute of all the other districts.

We can, then, say that the political unit, which finally resulted in the famous *pax Minoica*, was realized very early—during Middle Minoan times. We need not picture a despotic and strictly military constitution. The peaceful character of the Minoan civilization, and the fact that the palaces at Malia and Phaistos are rebuilt, prove that the constitution was a loose one, just firm enough to retain the whole island under one chief ruler. The peaceful and enterprising people of Minoan Crete—mainly merchants and sea traders—soon saw the advantages of a controlling power which made it possible for them to devote themselves with carefree confidence to their own sea enterprises. We find wholly analogous conditions in Mesopotamia which had one chief ruler, Lugal, and other lesser rulers, the so-called Patesi, each of whom was subservient to Lugal but wielded a certain amount of independent power.

All the Cretan palaces were rebuilt during MM III. Those of Phaistos and Malia lasted until the end of LM I, when they were destroyed. In the earthquake-suffering district of Knossos we have an intermediate catastrophe about the end of MM III but the palace was immediately rebuilt. It is obvious that we must attribute to this

⁷ Cf. the figures in *Palace of Minos*, I, 232 fig. 174-5.

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event the rich painting of the walls of this palace with frescoes, a means of decoration which was introduced at that time. In the other palaces we have no frescoes of this period.

The fresh catastrophe of LM I was fatal and general throughout the whole of Crete. It seems certain that it was the most terrible of all which occurred on the island. The palace of Knossos had, as we have seen, just been rebuilt and it may in consequence have suffered less than the others. It is, in any case, the only one that was repaired and continued in use into the succeeding period of the 'Palace style' (LM II). The other two palaces were destroyed, but that is not all. The same tragedy befell all the so-called mansions, such as those at Hagia Triada, Tylissos, Nirou Khani, Sklavokampos, Amnisos and Apodoulou—to mention only those already excavated. Whole cities, too, were destroyed. Gournia and the town of Knossos are typical examples but there were also Palaikastro, Pseira and Zakro. Even sacred caves fell in, like the one at Arkalokhori.

Here we must interpolate a few words about the ceramic styles of this time, in order to make it clearer that this great catastrophe was everywhere simultaneous. The LM I period is divided into two parts—the so-called 'floral style' period (LM Ia) and the 'marine style' period (LM Ib). It is generally thought that these two periods succeeded one another, but that is not quite true. Perhaps it is not true at all, for, in the frescoes from which the potter drew inspiration, the floral style is contemporary with the marine style. We have, moreover, vases painted in the marine style, such as the jar with dolphins from Pachyammos, in MM III, and furthermore the antecedents of both styles, both plant motives and marine motives, are already to be found at the height of the Kamares style in MM II.⁸

It is apparent, therefore, that in the vase-painting of both periods the floral and marine styles exist side by side, at least in their general lines, and are contemporaneous. Thus in the different ruined centres we have mentioned the greater number of examples (and the

⁸ For the jar from Pachyammos, *P. of M.*, I, 608, fig. 447. Vases of the Kamares style decorated with floral motives are very common, especially from Phaestos: Pernier, *Festós* I, pls. XVI, XX, XXI–II, etc. Marine motives, too, are common enough. Fish are already found on MM I vases from East Crete (*P. of M.*, I, 182, fig. 131). For an octopus on a vase from the cave at Kamares, *op. cit.* I, 246, fig. 186 (f). Seashells in combination with rockwork are known, e.g. *Festós* I, pl. xxx. Cf. the well known flying fish and sea creatures in faience from the Temple Repositories at Knossos, *P. of M.*, I, 520, fig. 379 and the gold goblet from the shaft graves at Mycenae, Karo, *Schachtgräber von Mykenae*, pl. CII, no. 73, which are works of the MM III period.

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more developed in style) of the marine class are found in East Crete (Palaikastro, Zakro and Pseira)—perhaps because the marine style was at home there. At Amnisos no vases of this type have yet been found but isolated examples occur at Gournia, Malia, Nirou Khani and Tylissos, together with vases of the advanced floral style—these last in great abundance. At Sklavokampos we found, together with vases of the advanced floral style, a jug decorated in the advanced marine style. The clay sealings which were found in the mansion at Sklavokampos are of the same style as those of Zakro and Hagia Triada. We even found—it was the first time such a thing had happened in Crete—a sealing identical with one from Hagia Triada, impressed by the same seal—a gold ring with an ellipsoidal bezel—on which a chariot was represented.⁹

There can, therefore, be little doubt that the great catastrophe overcame the whole of Crete at one and the same time. It has been suggested that Knossos may have destroyed the other centres. This is, however, scarcely plausible, for Knossos itself did not escape. It too was destroyed, together with its harbour town, Amnisos, and the whole surrounding district. Nor can we believe in an invasion from abroad. It is still premature to suggest an assault by Achaeans or any other invaders. If that were the case, we have yet to explain why Knossos, the only place to escape complete ruin, remained unfortified afterwards.

Thus the only remaining explanation of the disaster is one of 'natural causes'. A normal earthquake, however, is wholly insufficient to explain so great a disaster. In all the many earthquakes known to us, there has never been such widespread destruction at one and the same moment. In another district of Greece extremely liable to earthquakes—Corinth—the last destruction of which is still very fresh in one's memory, we find that Old Corinth was destroyed and New Corinth was built six kilometres away. Then the new town was destroyed and the old village remained intact. We know, too, that at the time of the great disaster of Helice, where the population was drowned to a man as a result of a great earthquake, the neighbouring Voura sustained serious damage but no more distant Aigion (40 stades) did not suffer at all.¹⁰

⁹ The mansion at Sklavokambos is still unpublished. For a preliminary report see *Arch. Anzeiger*, 1933, pp. 288 ff. and cf. Karo, *Altökretische Verwaltungskunst* (Eis Μνήμην Σπ. Λάμπρου), pp. 569 f.

¹⁰ Paus. VII, 25, 8. Cf. Senec. *Quest. nat. lib.* 6, 25.

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We could cite many other examples of the same sort of thing. When, therefore, we remember that Crete is made up of many, very varied, geological deposits and that it is only the soil of the Knossos district which is alluvial and prone to earthquakes, we cannot believe that a simple earthquake could cause so terrible a disaster. It was, therefore, all the more gratifying that the excavations at Amnisos furnished new evidence on the subject and led me to think that the disaster in Crete must be attributed to a tremendous eruption of the volcano on the neighbouring island of Thera.

This eruption is the greatest which can be proved historically. Briefly the story of the volcano on Thera is this. In geological times there were only two bare, rocky islands composed of non-volcanic stone. Their present names are Hagios Elias and Monolithos. Gradually several craters were formed under the surrounding sea and by their continual eruption of volcanic matter a round island was built up, incorporating the two original rock-islands. The volcanoes were dormant for a time, and the whole of the island was covered with vegetation. We know that at that time myrtles, olives and palms were among the flora, and that the island was inhabited. There was, in fact, more than one settlement on it. Then suddenly the sleeping volcano awoke and, after a tremendous paroxysm, the whole of the centre of the island, together with a part of the west coast, was blasted away and sank—in all an area of 83 square kilometres. What remained was Thera, Therasia and Aspronisi. In the centre a great abyss was formed, originally more than 600 metres in depth—the greatest and most imposing caldera in the world.¹¹

Before the final eruption a great quantity of ashes and pumice stone, thrown up by the volcano, covered the island with a layer thirty metres thick—a phenomenon which is typical of all the great explosions of Thera. We know from the Byzantine chronographer Theophanes that during the eruption of A.D. 726, in the reign of Leon Isaurus, pumice stone covered a great area of the sea round about and floated afterwards as far as Asia Minor, Abydos, Salonika and Macedonia. Similarly, during the eruption of 1655, of which we have more than one description, ashes and pumice stone and even the sound of the explosion reached Asia Minor. In Chios they thought that a naval battle was taking place in the neighbourhood between the Venetian and Turkish

¹¹ See the important book by Fouqué, *Santorin et ses éruptions*, Paris 1879. The geological processes of the great eruption are a little modified by Hans Reck, in *Santorin*.

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fleets. During the first mentioned tremendous eruption, which destroyed the island, many houses were buried under the layers of pumice stone. Some of these have been excavated, in particular by Gorceix and Mamet of the French School in Athens. They found vases, implements and pieces of fresco¹²—all belonging to the First Late Minoan period. The vases are of local manufacture but they imitate Cretan prototypes and thus we can safely date the explosion to the last years of LM I, about 1500 B.C.

No historical account survives of this great earthquake, but fortunately we have an excellent means of reconstructing all the phenomena which accompanied the disaster, in the eruption of Krakatau in the Dutch East Indies on 26–27 August 1883. Geologically speaking, both volcanoes belong to the same family, and the phenomena of their eruptions are therefore analogous. The islet of Krakatau is much smaller than Thera and the part of it which was submerged was about a quarter of the other (22.8 sq. km. against 83 in Thera). The account of the events and disasters which accompanied that eruption is truly amazing.¹³ Vast quantities of pumice covered both the island and a great part of the sea round about. A whole island, Calmeyer, was formed in the strait of Soude and a harbour in the neighbouring island of Sumatra, that of Lampong, was so blocked up that for several months sea communication was broken off. A tremendous roar accompanied the explosion and was heard over 2000 miles away—just one twelfth of the earth's circumference. The vibration of the atmosphere set up by this noise broke window-panes and cracked walls at places as much as 100 miles away and even further, and these air waves encircled the earth several times. Such quantities of volcanic ash filled the air that, even at a distance of 100 miles, day was turned into dark night, and these ashes fell as much as 1000 miles away. Very fine particles of ash were thrown 30 miles up into the stratosphere, picked up by the air currents and dispersed over the whole of the earth. This dust was suspended in the air for months and months, and the wonderful colourings of the sunsets in 1883 were ascribed to it. But worst of all was a series of terrific waves which rose after the explosion. They were as much as 90 feet high, and broke with devastating force and

¹² The vases are now in the French School at Athens. They have been published by Renaudin, *B.C.H.*, 1922, XLVI, 113 ff. On the frescoes see Perrot and Chipiez, *Histoire de l'Art*, VI, 538–9 figs. 210–12. The flowers represented upon the stucco fragments are lilies of an already advanced LM I style.

¹³ R. D. M. Verbeek, *Krakatau* (Batavia, 1886) in Dutch and French editions.

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speed against the coasts of Java and Sumatra. Where they struck a plain, they swept inland, and as far as 1000 yards inland they were still 15 yards high. Whole towns, villages and woods were destroyed, and great masses of stones from the sea were hurled far inland. So, too, trains and ships. One of them, the steamer Barouw, was found afterwards beyond the town of Telook-Betoeng several kilometres inland. This amazing catastrophe cost over 36,000 lives.

The distance between Thera and Crete is only about 62 miles. It is certain, therefore, that the inhabitants of Crete in 1500 B.C. lived through the same moments of terror as did the inhabitants of Java and Sumatra in 1883. If the explosion took place during the day, the day was surely turned into night and much damage was caused by the tremendous vibration of the air. The thunderous roar, too, must have deafened and terrified the Cretans, who had, of course, no means of knowing what was its cause. Then must have come the rain of mud and ashes, some cold, some ablaze and burning. Worst of all, however, were the waves which broke over the island, much higher and more rapid than those at Krakatau. For on the basis of his observations of the phenomena Verbeek has worked out a mathematical table, from which one can see that the speed of such waves is in proportion to the depth of the sea at each point. This is shown in the following table, which is based on observations at three different points on Sumatra, giving their relative distance from Krakatau and the respective depths of the sea.¹⁴

Place	Distance from Krakatau	Depth of sea	Speed of wave per second
Sanganila	64.00 km.	43.9 m.	25.96 m.
Beneawang	110.40 km.	109.5 m.	36.70 m.
Vlakke Hoek	103.00 km.	150.0 m.	41.17 m.

The sea between Thera and Crete is incomparably deeper than in the strait of Sonde. According to the excellent chart¹⁵ of Imray, Laurie, Norie and Wilson, which I have before me, the sea here reaches a depth of 1005 and 1020 fathoms, *i.e.* about 2000 m. Even near the coast the depth is 150 and 400 fathoms at different points off Thera, and 96 and 116 off Crete. At a point exactly 26 miles from Amnisos it is as much as 1100 fathoms deep. (I do not know whether the composition of the sea floor at several points where it is marked 'yellow mud pumice stone' may be ascribed to volcanic matter from Thera).

¹⁴ *op. cit.*, p. 400.

¹⁵ Imray, Laurie, Norie and Wilson (London), no. 149c.

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There is, therefore little doubt that waves caused terrible destruction in Crete at that time. Mathematicians can calculate exactly the speed of the waves on the basis of Verbeek's algebraic tables; but, if we take only the very low speed of 50 m. a second, little more than half an hour sufficed for the inundation of the Cretan coast; and the speed was certainly much greater than that.

All the settlements on the coast soon disappeared—Amnisos, Nirou Khani, Malia, Gournia and Zakro, perhaps too Pseira and other towns. (This depends on the height of the waves). The finds in these centres prove that they were destroyed at one and the same time. At Nirou Khani the inhabitants had no time to take anything away. Huge double axes, the sacred symbols, lay where they had fallen. In one room the excavators found dozens of offering tables stacked one on top of another, as they were stored at the time of the disaster.¹⁶ At Gournia the carpenter's and coppersmith's shops were found intact.

The most instructive excavation, however, was that of Amnisos. One of the buildings there, which lies quite near the sea, gave us the clue to the cause of the great disaster. The deeper levels of it were buried under a great mass of pumice stone and sand. A square pit inside the building near the southeast corner was literally full of pumice stone. We did not at first appreciate the significance of this fact and in my provisional report on the Amnisos excavations I asked whether the pumice stone were as traders' wares, purposely stored in the pit, though Santorin and the eruption came to my mind.¹⁷ Later, however, the real explanation occurred to me. We can, in fact, reconstruct the phases of the disaster. When the waves broke the building was carried away almost to its foundations. Then it lay abandoned and after a time the north breezes brought the pumice stone from the volcano as far as Crete. As this building stands on the shore, it was constantly inundated by the sea and the rooms were thus covered with pumice stone. That this certainly was the case is proved by the fact that the pieces were all small, rounded, and polished like pebbles. Similar pieces—in small quantities—are to be found today on the Greek coasts. They fall into the sea at Thera and are carried everywhere by the waves.

The excavation of the 'Villa of the Frescoes' at Amnisos was equally instructive. Lying as it does a little farther inland and higher

¹⁶ *Αρχαιολογική Έφημερίς*, 1922, p. 1-25.

¹⁷ *Πρακτικά*, 1932, pp. 79 f.

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up the shore, it was not reached by the pumice stone. During the excavation, however, we noticed the unusual way in which the walls and the corners of the rooms had fallen in. Undoubtedly a tremendous natural force had caused this devastation—surely the waves after the eruption. As these waves receded, their strength was so great that they prized huge orthostatic blocks out of position and made the walls bulge outwards.¹⁸ This is on the west side, at right angles to the line of the coast. On the north side, parallel to the waves, where the impact was much greater, two orthostats (or perhaps a single one of great size) are now missing and undoubtedly were carried away by the waves.

A further point proves how theoretical arguments are often insufficient to explain the facts. In the villa at Amnisos where, as so often in Minoan architecture, wood was used to a very great extent, we find that, especially on the north side, where there was a hall in both stories, there were traces of intensive fire. The stone bases of the wooden pillars were much damaged and blackened and rendered very friable. This was a great problem, as I could not reconcile the fire with a terrible inundation caused by the sea. I then found that exactly the same phenomenon was observed at Krakatau. 'At Tjaringin the waves swept away the houses . . . between seven and nine o'clock. The waves overturned the houses on the coast and the ruins were set on fire by the little lamps. The fire was repeated three times'.¹⁹ We can, then, suppose that by the time the waves from Thera reached Crete it was night, or the day had been transformed into night and the inhabitants had lit the lamps—exactly as happened in the case of Krakatau.

To complete the picture of terror and dismay we must mention a few more characteristic phenomena at Krakatau. As all these were perceptible as far away as 100 and 150 km., we may justifiably suppose that similar conditions prevailed in Crete—to an even greater degree. The atmosphere in Krakatau at that time was electrified so that lightning struck people, and buildings such as lighthouses. A terrible gale sprang up with a deafening roar. Burning volcanic ashes, which blistered and killed people, rained down. At intervals soil and pumice stone fell. Pieces of pumice stone as big as a man's head fell 80 km. from Krakatau. This means that they must have been ejected at a speed of more than 1070 metres a second and have risen to a height of 50 km. Lamps were torn from their supports at Buitenzorg in Java,

¹⁸ One of these orthostatic blocks is two metres long and one metre high.
Cf. Πρακτικά, 1932, p. 92, fig. 10.

¹⁹ Verbeek, *Krakatau* (French edition), p. 46.

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150 km. from Krakatau. We have already mentioned that window-panes were broken equally far away. Old houses in places as far as 830 km. from Krakatau were so cracked that they had to be abandoned.

I think there is little reason to doubt that the devastation of the coast-sites of Minoan Crete was caused by the waves from the eruption of Thera. We read of similar phenomena on the occasion of later eruptions of the same volcano, which were in every case less violent than the one in 1500. Philostratus, for example,²⁰ reports that during the eruption in A.D. 60, when the islet Hiera appeared in the caldera the sea receded about seven stades from the south coast of Crete, at the point where the Asclepieaeum of Lebena was situated. The terrified priests expected that the waves on their return would sweep the sanctuary away but Apollonius cried 'Take heart, for the sea bore earth'. Similarly it is reported that in the eruption of 1650 the waves in Ios rose 50 feet; at Sikinos the sea came 350 feet inland, while in Thera itself 'the waves came two miles inland and swept away old walls and chapels, foundations and all'. In Crete the ropes with which boats were tied up were cut through.

It is difficult to say, in the case of Crete, how far inland the waves came during the eruption of 1500 B.C. In the case of Krakatau we know definitely that 1600 m. from the coast the waves were still 15 m. high. Elsewhere the sea came two km. inland. Here the waves were only three metres high but they advanced with such force that widespread damage was caused.

It was only in the plains, however, that the waves came so far inland. The Cretan palaces, therefore (with the exception of that at Malia, which was almost certainly destroyed by the waves) and the other inland settlements could not possibly have been reached by the waves, even if we suppose that they were much higher than at Krakatau. It is only by a series of violent earthquakes that they can have been destroyed.

This put a new difficulty in the way of those who believed in a simultaneous destruction of all the sites of Crete, since Fouqué and Verbeek agree that eruptions such as those of Krakatau and Thera are never accompanied by earthquakes. This was proved in the case of Thera by the fact that the prehistoric buildings buried under the deep layers of pumice had their walls intact to a height of several metres. It is, therefore, impossible that earthquakes accompanied the eruption.

²⁰ Philostratus, *Vita Apollonii*, 34.

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Moreover, during the eruption of Krakatau only insignificant shocks were felt.

New light was thrown on this problem by a series of articles by Professor N. Kretikos, Professor of Seismology in the University of Athens. Verbeek had already noticed that before and after the eruption of Krakatau earthquakes were felt more often than usual in all the districts round about. Some of them were serious. According to Verbeek, some of them, particularly those which took place after the eruption, were possibly to be attributed to it.²¹ Professor Kretikos has developed the theory more systematically,²² and shows that before and after every eruption of Thera a whole series of earthquakes is to be noted in the neighbourhood: in Crete, the Cyclades, Sporades, Dodecanese and even farther afield. This was what happened in the great earthquake of 26 June, 1926, which beyond doubt was connected with the eruption of Thera some months earlier. It caused considerable damage not only in Crete but in many other districts—in the islands of Karpathos, Kastellorizo and especially in Rhodes, where thousands of houses were destroyed with loss of life. In Anatolia, too, houses and mosques collapsed. Damage was widespread throughout Egypt, with the exception of Assouan, and in Alexandria and Cairo alone 600 houses collapsed.

History corroborates this theory of Verbeek and Kretikos. We know, for instance, from various sources (Strabo, Pausanias and Plutarch) that in 197 B.C., when the island of Hieria appeared, serious earthquakes devastated Rhodes and Asia Minor. It is reported, too, that a great earthquake took place a year before the eruption of A.D. 726. Theophanes put the responsibility for all the disasters at that time upon the shoulders of the iconoclastic Emperor Leon. So, too, it is reported that very serious earthquakes took place a year before the eruption of 1650.²³

We may be sure, therefore, that the same thing happened on the occasion of the tremendous eruption of 1500 B.C.—either as a prologue or, more probably, as an epilogue. A series of violent shocks may have devastated all the neighbouring districts, until the disturbed layers of

²¹ Verbeek, *op. cit.*, p. 463.

²² ' Sur la sismicité des Cyclades et de la Crète ', *Annales de l'Observatoire d'Athènes*, 1925, IX, 36; ' Sur les phénomènes sismiques produits avant et depuis l'éruption du volcan de Santorin ', *l.c.*, 1926, VIII, 2 ff. Cf. also *l.c.* 1928, X, 47 ff. and *Comptes Rendus de l'Acad. des Sciences*, vol. 181, p. 923.

²³ See Fouqué, *op. cit.* and B. 'Ακύλας, *Τὰ ἡφαίστεια καὶ ἡ νῆσος Θήρα* Athens, 1925.

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the earth had settled again. In Crete, especially, earthquakes may have completed the devastation which occurred.

This is neither the time nor the place to examine the possibility that devastation elsewhere—in the Cyclades (Melos), the other islands, in Asia Minor and even in Egypt—should be attributed, directly or indirectly, to the great disaster of Thera. Let us confine ourselves to Crete, where this terrible disaster had a profound influence on the culture of the island. It received an irreparable blow, and from then onwards gradually declined and sank into decadence, losing its prosperity and power. What power remains is concentrated at Knossos which, for a little while, carried on the venerable traditions of the Minoan civilization as the 'Palace Style'. The other palaces are not rebuilt, and the 'Palace Style' is to be found only sporadically here and there outside Knossos. There is no longer any town or any great centre in Crete. The culture and art faded steadily and finally died out completely.

We can be certain that after the great catastrophe the majority of the inhabitants fled in terror from the island. They thought that the mother-goddess had turned against her island and cursed it. Anyone who wants to picture the terror and agony of the islanders during the eruption of Thera should read the personal impressions of a scholar so calm and rational as Verbeek, as he recorded them during the eruption of Krakatau. Although he was at Buitenzorg in Java, 150 km. away from the volcano, he recounts that there, too, as in Batavia, which was equally far away, singularly dramatic events took place. During the whole night no one in western Java could sleep for the roar and the flashes from the eruption. Anyone who did not witness it can scarcely imagine the feeling which comes over a man when a mountain which lies 150 km. away utters a roar as terrible as the salvoes of guns fired in the immediate neighbourhood. In addition, all the objects which were free and loose were continually being moved about; as a result, crashes, bangs, clashes, creakings and crackings resounded everywhere, causing physical and mental agony. The crashes were followed by complete silence, which was equally terrifying and caused widespread nervous hysteria. In the morning a horrible noise broke out so that the men who were attempting to make up for their lost sleep leaped from their beds. Lamps fell from their fittings and plaster from the walls; doors and windows were thrown open; complete pandemonium reigned. Then it began to grow dark, increasingly rapidly after ten o'clock and the lights were lit. A cloud of yellowish grey smoke came

over the land. Everywhere mists came down—cold and damp and bringing a slight smell of sulphuric acid. The domestic animals were as frightened as the people. They stayed near their owners and near the lights and refused to go away even when dragged. About two o'clock a dim light appeared in the East and the cocks began to crow. . . .²⁴

Given that the eruption of Thera was more violent, and its distance from Crete less, than even described, there is little doubt that the Cretans lived through moments of equal, if not greater, terror. When, moreover, great earthquake shocks followed, it was quite natural that some of the survivors abandoned the cursed island in search of new homes. May we suppose that the Keftiu of the Egyptian frescoes were Cretan refugees who settled in North Syria or somewhere thereabouts? And does the tradition that Crete remained devoid of inhabitants from that time reflect some vague reminiscence of this event?²⁵ The period of the 'Palace Style' suggests that a fairly powerful dynasty still reigned at Knossos but it already looks as though Crete were no longer invulnerable. It would appear that the Achaeans first began to descend on Crete. It is now that we note a considerable change in the hitherto peaceful character of the Cretan people. The first graves of warriors furnished with swords and arrows date from this time and the tablets of Knossos present lists of armour, horses and chariots.

In time Crete sank further and further into decay. After 1400 even the palace at Knossos was deserted and simultaneously the great period of Mycenaean expansion began, when colonies were established in Rhodes, Cyprus, Asia Minor and elsewhere. It is not possible that Crete was excluded from this great movement by the Achaeans, and to this are due the Achaean place-names: Μυκῆναι, Ἀμύκλαι, Θεράπνη, Ἡλέκτρα.²⁶ We need not imagine a wholesale conquest of Crete. It may have been mere colonization, carried out without violence, which resulted in strong trade connexions with the mainland. This explains why Cretan art and even Cretan writing—with the same combinations of letters found on the tablets at Knossos—occur on the Greek mainland. *Mutatis mutandis* we could repeat the well-known phrase *Creta capta ferum victorem cepit*.

A civilization of 2000 years' standing does not disappear without leaving a trace. The Dorians found many products of an advanced civilization, and therefore progressed quickly. They learned, for

²⁴ Verbeek, *op. cit.* 33-40.

²⁵ Herod VII, 171.

²⁶ A. Fick, *Hattiden und Danubier*, p. 9.

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instance—alone of all the Greeks—the science of archery;²⁷ to keep slaves under more humane conditions than existed anywhere elsewhere in Greece; there they found music and poetry. Finally, in the island of Minos, the great law-giver, they developed the famous laws which were later borrowed by their fellow tribesman on the mainland. Who knows how many of the clauses of the famous inscription of Gortyna are of Minoan origin? As for the religion, it has already been proved that most of its elements are derived from the Minoans. The Dorians in Crete continued to frequent the caves which had been sacred to the great Minoan goddess. It seems as if the race were not changed and as if no other change took place later. No break in the cult is perceptible from the height of Minoan times to the last days of Graeco-Roman antiquity. Many of these caves are already known and more are continually coming to light.

NOTE.—The Editors wish to point out that in their opinion the main thesis of this article requires additional support from excavation on selected sites. They hope that such excavations will in due course be carried out.

²⁷ Of this problem, which had already drawn Pausanias' attention (1, 23, 4), I think Minoan tradition provides the only reasonable explanation. The Minoans are represented as energetic archers and huntsmen from the Early Minoan period to the very end of Minoan times.

A Bird's-eye View of Opis and Seleucia

by CLARK HOPKINS

Director of the Michigan Excavations at Seleucia

IN 1927 Professor Waterman of the University of Michigan undertook a special study of Babylonian topography for the purpose of locating the site of Opis (Babylonian Upi, Upa) the older Sumerian Akshak, capital of one of the oldest kingdoms in Mesopotamia.¹ The general location of Opis is well defined by the inscriptions of Nebuchadrezzar at Wadi Brissa and Nahr el-Kelb, who states that for the defence of Babylon he built a wall from Sippar on the Euphrates to 'above' Opis on the Tigris, a distance of five *beru*, about twenty miles. Since with the help of this brick-faced earth wall he fashioned great bodies of water 'like the sea', twenty *beru* in length (probably in circumference), it is clear that a watercourse formed part of this system of defence. Xenophon mentions Opis as located above the modern Bagdad, but Strabo clearly places it in the district of the later Seleucia, stating² that the Tigris was navigable as far as Opis and Seleucia. In another passage³ Strabo, following the geographer Eratosthenes, mentions the ancient wall (the wall of Semiramis, he calls it) as being in the narrowest part of Mesopotamia, and Opis as 200 stades (about twenty-three miles) distant from the Euphrates. Since the present distance from Sippar, whose location is known, due east to the Tigris at Ctesiphon, is between twenty-one and twenty-two miles, it seemed clear that the mounds of the ancient city lay on the west bank of the Tigris, across the river but not far distant from the later Sassanian capital. For the Hellenistic period Pliny offers the information⁴ that the Greek capital of Seleucia was founded at the confluence of a canal and the Tigris: *in confluenta Euphratis fossa perducta atque Tigris*, and Ptolemy⁵ mentions a royal river, βασιλῆος ποταμός, as starting from Sippar (Σιπφάρα), flowing through Seleucia and entering the Tigris at Apameia. If this ancient

¹ Leroy Waterman, *Preliminary Report upon the Excavations at Tel Umar, Irak*, University of Michigan Press, 1931, pp. 1 ff.

² XVI, 1, 9.

³ II, 1, 26.

⁴ VI, 5, 89.

⁵ V, 17, 5.

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royal canal paralleled very closely the course of the wall and water courses of Nebuchadrezzar, Opis and Seleucia should lie close together or perhaps are to be identified as one and the same. Particularly significant, therefore, seemed the extensive remains of an older canal observed by the Iraq Department of Irrigation and especially by A. Gray, British engineer in charge of the new canal. The older canal, lying close beside the royal canal (Nahr Malcha) of more modern times, turns sharply south before reaching the river to discharge itself into the Tigris some distance below Ctesiphon; but in Hellenistic and pre-Hellenistic times the stream could easily have reached straight forward to the river above Ctesiphon or, with a very slight turn to the south, to the bank immediately opposite Ctesiphon, where we know that Seleucia lay.

For these reasons the district along the river opposite the great arch which marks so signally the site of ancient Ctesiphon, was scrutinized with particular care for traces of the lost city Opis. Almost immediately opposite the arch of Ctesiphon lie two great mound complexes, the one separated from the district around the arch only by the river, the other about a mile from the river bank and slightly north as well as west of Ctesiphon. If Seleucia lay directly across from Ctesiphon then the mounds further from the river hid an unknown city. And a city of importance it was, for the high ground stretched for half a mile and more to either side of the Arab hill of Tell Umar which marked the north centre of the mound, and back into the plain for a mile and a half in a great rectangle. Professor Waterman perceived at once that the river must at one time have washed the edge of the city, for only the river could account for its size and importance. If there was some question whether this mound concealed the old Seleucia rather than Opis, doubt was removed by the investigations of German scholars, who agreed in placing ancient Seleucia on the banks of the Tigris, and who in 1927, desiring to uncover Seleucia, requested permission to excavate the mounds directly across from the modern Ctesiphon.

Even before excavations began, however, air-photographs of the district, taken by Wing Commander Insall, v.c., stationed at Bagdad, revealed the real state of affairs. From the air a regular pattern of streets was clearly visible, cutting a large part of the Tell Umar complex into a series of rectangular blocks in the Greek (Hippodamean) plan. This was strong evidence that the Greek city actually lay in the Tell Umar complex and that the mounds nearer the river were actually only a part of the ancient Ctesiphon. Excavations very quickly proved

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the truth of this hypothesis.⁶ The river had changed its course not from Opis to Seleucia, but from Seleucia to the middle of Ctesiphon, so that only half of the great Sassanian city now lies on the east bank.

If, however, the Tell Umar mounds embraced Seleucia, they did not necessarily enclose Seleucia alone, and Professor Waterman very reasonably suggested that Opis lay beneath the whole or a part of the Greek city. The absorption of Opis by Seleucia would account for the disappearance of Opis as a city and would explain the complete separation of the tradition of the name from the site. Excavations were therefore continued by the University of Michigan, under the direction of Professor Waterman, but with some alteration of plans in recognition of the fact that the Greek city must be excavated at least in part before any more ancient site could be uncovered.

Work of the first seasons at Seleucia was devoted in part to trial trenches designed to establish the identity of the site, to discover the depth of ancient strata and if possible to locate remains of the older city Opis. In addition, near the centre of the city, a block distinguished in the air views by more prominent mounds was chosen for systematic excavations. When in the 1936-37 season new areas for excavation were to be determined upon, a review of the topography of the site in the light of the first excavations became imperative. For this new task air views were of supreme importance. We were fortunate indeed in obtaining through the kind offices of Major MacDonald, who held a command in Bagdad, splendid air-views of the site from the collection of the Royal Air Force. Photographs taken shortly after rains, before the earth was completely dry, revealed more sharply than before the outline of blocks, and views taken with the sunlight slanting across the mound brought into sharper relief ridges and depressions within the city.

The network of streets forming rectangular blocks is most distinct in the centre of the city in the vicinity of the block excavated. Reuther saw in the early air-view two main streets crossing one another in the centre of the metropolis. In reality one of his main streets, that running in general north to south, is a late canal, probably of Sassanian times, which follows only in part the line of an ancient street through the city and opens into the Tell Umar area. Actually there were two great thoroughfares running in parallel lines through the city from the west and dividing it into three parts. Between these

⁶ ANTIQUITY, 1929, III, 434-51, 'The German Excavations at Ctesiphon', by Oscar Reuther.

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two avenues or canals the area is almost level, and the block lines are most easily distinguishable. Since excavations in the block revealed a Parthian palace overlying a group of houses, it seems reasonable to assume that the block space between the two thoroughfares comprised the chief residential district of the city.

The most prominent feature in the north section of the city is the mound of Tell Umar located in the centre of this side of the site and partly cut on the northeast by river floods. It is an artificial mound made of a great circular mud-brick wall filled with loose earth and debris, except in the centre where a small square tower of mud-brick was erected. It was constructed in its present form apparently in early Sassanian times but for what purpose partial excavations have not revealed. Possibly it was the tomb of a prince, or perhaps a great altar reminiscent of the ziggurat. Ammianus saw on the Euphrates at the entrance to the Royal Canal a lofty tower like a lighthouse.⁷ Possibly at Seleucia in Sassanian times, where one branch of that great canal met the Tigris, a second tower was erected to serve as lighthouse and watch tower.

Whatever the purpose of this tower may have been, it stands in an area distinctly lower than the level of the residential district and the surrounding mounds, an area, therefore, which must have been distinguished even in the older Hellenistic city from the normal block-system.

At the northeast corner of the site where the rectangle of the city lies closest to the oval enceinte of Ctesiphon probably stood, as Reuther suggests,⁸ one end of the stone bridge mentioned by ancient authorities as connecting Seleucia and Ctesiphon. Between this bridge-head and the east section of the more northern of the two thoroughfares, there may be remarked on the air-views a rectangular depression comprising several blocks. In the centre of this low area a small oval mound stands out rather prominently. Less distinct is a rather similar area on the other side of Tell Umar, an area which adjoins the northern thoroughfare and is almost equidistant from the centre of the low area around Tell Umar. In the little mound in the northeast corner excavations undertaken as a result of this survey have revealed a Parthian temple and small theatre for religious rites standing in the middle of a great precinct. It seems clear that these remains rest above an earlier Greek temple of baked brick, though excavations have only touched these earlier remains. In the second rectangular area

⁷ XXIV, 2, 7.

⁸ *ANTIQUITY*, 1929, III, 439.

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the mound has yielded a second small theatre, but the arrangement of the temple has not been established. The relatively low rectangular areas around these mounds are accounted for by the great precincts obviously assigned to the temples early in the history of the city, and so kept clear of the building and rebuilding in successive epochs. Since this is so it seems plausible to assume that at least a part of the low area around Tell Umar is due to a similar precinct. Excavations in the vicinity of Tell Umar have not brought to light as yet a temple, but have disclosed a number of finds which suggest religious worship.

Undoubtedly many temples were erected in the vast city of Seleucia, but as far as we can judge from the air-view of the city at present the chief sanctuaries lay in the north quarter. In the Hellenistic capital of Pergamum, the chief temples and the palace were placed on the acropolis, which lay above the residence-blocks and the market-place. In the Hellenistic commercial cities, the market place in the form of an open rectangle was situated near the port or along a thoroughfare of traffic, and the private houses and temples divided the rest of the city proper. If in Seleucia the residential section lay across the centre of the city, and the chief temples were erected in the north section, then perhaps we may look in the south quarter of the city for the civic centre, which would include the market place, the senate house and record offices.

Here in the south sector we find in fact a broad east-west thoroughfare bordered with irregular mounds of considerable size. Not far from the centre of the city, on the north side of this thoroughfare, lies an open area occupying the south ends of two blocks and surrounded by impressive mounds on three sides. In shape it corresponds to the rectangular form of a Greek market-place, and its position with open side on a main line of traffic corresponds exactly with our information of the location of Greek agoras. Further evidence that it was at least an important centre may be adduced by the width of the street, which debouches into the middle of the north side of the area. This street, at the point where it passes the block excavated, is almost three times the breadth of any one of the other three streets which circumscribe the block. Surely then this north-south street was a great artery of traffic and presumably led to a civic centre of importance.

The rectangular open place or market is faced across the thoroughfare by a semicircular mound which suggests the outlines of a civic theatre. A small trench in the north corner of this mound had in previous seasons disclosed a series of three doorways of baked brick

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separated by small sections of walls adorned with double engaged columns. The gates themselves are fifteen or twenty feet above the last ground level of the city and rest on mudbrick and débris. Quite possibly, then, they formed part of a theatre entrance, giving access to the higher seats and approached either by a sloping ramp or staircases which have now disappeared. In some of the mounds round about may be located the senate house, the law courts, and other buildings of the civil administration. Possibly to the large oval depression northwest of the market square the stadium and gymnasium should be assigned. Our excavations are far from complete enough to identify individual buildings, but every indication in a broad survey points to a series of large non-residential buildings in the southern sector of the city and it is these buildings which would presumably have formed the civic centre.

One more observation might be made in respect to this quarter. The old bed of the Tigris touched the northeast corner of the city and then swung along its east side, where presumably, as Reuther suggested, the harbour should be located. The two great thoroughfares of traffic, running straight through the site from west to east, both therefore end at the port. The air-photographs show that the banks or sides of these two avenues are quite different in appearance, for along one there is a great series of irregular mounds, along the other the ground is for the most part level, rising little if at all above the artery level. Could this difference be due to the fact that one was a canal, the other a broad street? We have Pliny's authority that Seleucia was founded at the confluence of the Tigris and the canal from the Euphrates. A branch canal from this stream would allow traffic from both rivers to enter the city. Many of the mounds along this waterway are presumably public buildings—the market, the theatre, senate house, etc.—but others may be merely dykes along the water-course or means to prevent flooding when the rivers rose. It seems significant that the great avenue through the city that best fulfils the specification for a waterway debouches at the south end of the harbour where one would expect the entrance to be, and the other thoroughfare leads to the district of the bridgehead to Ctesiphon.

After stating the location of Seleucia Pliny goes on to say that its inhabitants numbered 600,000 and that in form the walls resembled an eagle with outstretched wings: *situm vero moenium aquilae pendentis alas*. Were he considering the city as a whole, one would be very much tempted to understand the figure of comparison not as the soaring

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eagle, but the standing eagle with high-shouldered wings thrust out to either side, as it is so commonly represented on Hellenistic and Roman monuments. The three broad divisions of the city fit admirably the eagle's body flanked with vertical wings; the curved breakwater of the harbour would form neck and head. The particular mentions of walls, however, may mean that the head should be located at the bridge and the wings are the lengths of walls along the river bank to either side. At present the mounds form almost a right angle at the south corner where the bridge lay; but in ancient times the angle was probably much less sharp, as the destruction of a part of Tell Umar itself by floods suggests.

The topography of Seleucia is of particular interest, not only because it was one of the greatest of Hellenistic capitals, named by Strabo⁹ with Alexandria, and ahead even of Antioch in size, but also because it is one of the few great Hellenistic cities laid out on flat ground. The architects of Alexandria were circumscribed by the limits of the island; the experts at Seleucia had the whole broad plain of Mesopotamia in which to lay the scheme of their city.

We can only admire the skill with which they performed their task. The residential district through the centre of the metropolis gave easy access to either of the two great thoroughfares through the site. The market-place and civic buildings were established along one of the city arteries; the chief sacred precincts were separated from the private houses by a broad avenue. If we may judge by Pergamum, the palace should be located close to the temples and presumably near the centre of that district. In the vicinity of Tell Umar, I am therefore inclined to place both the palace and a temple. From the palace might run a north-south artery leading southwest to the ancient Babylon. Not only, then, do the carefully drawn streets form a regular network, but the whole city exhibits the balanced plan of a master architect. The great skill in city planning which must be assigned to Hellenistic architects in view of excavations in lesser places is perhaps one of the best reasons why we may expect the logical scheme one sees outlined in the air-map to emerge eventually as the actual plan of the ancient city.

One might object that we are basing the scheme of the Hellenistic city on air-views which at best show only the aspect of the site after some centuries of Parthian and perhaps Sassanian domination. This is true, but our excavations have revealed thus far very little change in

⁹ XVI, 2, 5.

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the general aspect of the city in the course of its history. In the block a major conflagration occurred in 143 B.C. just before the city was taken by the Parthians. In A.D. 60-70, after the city had lost much of its autonomy, the style and plans of houses were radically changed. The city was sacked in 116 by the army of Trajan, in 165-66 by the soldiers of Verus, and again just before 200 by Septimius Severus. In the whole long course of its history the outlines of the excavated block remained the same except for slight widening or narrowing of the surrounding streets. The low ground of the temple areas bears witness to the great length of time during which these precincts were kept clear of building, and their great size suggests their allocation to the gods before the city was crowded with residences. Our evidence is very strong, therefore, that despite the city's many vicissitudes of fortune the general plan remained intact.

One fundamental problem remains unsolved: the location of the ancient Opis. A few Babylonian seals have been recovered from Seleucia; many baked bricks re-used in building bear the stamp of Nebuchadrezzar; the minor finds include a number of items which belong to a pre-Hellenistic era; nowhere is there clear evidence of occupation before the Seleucids. Possibly the location of the city gradually shifted, following a gradual change in the river bed. Some air-photographs show that the block system extended beyond the present mounds into the irrigated land southwest of the mounds of the city. Even here the surface sherds do not justify the hypothesis of an early Babylonian town.

To locate Opis and Seleucia on the same ground required the identification of the ancient wall of Nebuchadrezzar running from Sippar to Opis with the Hellenistic canal which ran to Seleucia perhaps from Sippar. This assumption is by no means certain, particularly since the two water-ways were built for quite different purposes. A good deal of doubt is placed upon the hypothesis, moreover, by remarks of Strabo. When Strabo states that the Tigris was navigable as far as Opis and Seleucia, the passage does not necessarily mean that they were separated from one another, but it does *suggest* that they were distinct, and moreover that Opis lay south of Seleucia. When, however, Strabo goes on to say that Opis was a village emporium for the surrounding district, ἡ δὲ Ὀπίς κῶμα ἐμπόριον τῶν κύκλῳ τόπων, he apparently differentiates it sharply from the much more important city of Seleucia. Nor would the close approximation of distance between Sippar and Opis on the one hand, as given in the inscriptions of Nebuchadrezzar and the account

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of Strabo, and the actual distance between Sippar and Seleucia on the other, cause serious difficulty, for, as the map shows, the distance to the Tigris from Sippar would be little increased if the wall ran to a point a few miles either below or above Seleucia. Air-views may show, moreover, that windings of the river in ancient times brought the Tigris actually closer to Sippar at other places than the point due east where Seleucia lay. We may hope that such an air-survey will reveal to us not only the old river bed, but show more clearly the course of old canal systems and perhaps even the straight line of the embankment of Nebuchadrezzar. We know the location of Sippar, whence the wall started—its length as given precludes many turns and presumably its course was straight; where it reached the Tigris, or immediately below it, lay Opis. From the air we should obtain a final answer to the problem. It may still lie beneath Seleucia, or it may lie some distance away, presumably towards the south. Possibly now variations in the river bed have even changed its location from the west to the east bank as they changed it for a period in the time of Tiglath-Pileser I.¹⁰

Many seasons of excavations are required in the great Mesopotamian capital of the Greeks and Parthians before we can speak with certainty of the city plans. It may be that these seasons will show our analysis of the arrangements of the city drawn from air-photographs to be quite erroneous. Meanwhile we can only recognise the tremendous assistance air-views have rendered our work in the past and look forward confidently to our campaigns in the future. At least we obtain from them the working hypothesis outlined above, an hypothesis which in itself is no mean advantage in so vast a site. I am happy indeed to have this opportunity of acknowledging our great debt to the Royal Air Force in Bagdad and of expressing our appreciation for the very cordial co-operation which all the members of that organization have unfailingly accorded our expeditions.

¹⁰ D. D. Luckenbill, *Ancient Records of Assyria and Babylonia*, Chicago, 1927, p. 309.

Notes and News

LOAN-WORDS IN SEMITIC LANGUAGES MEANING 'TOWN'

In *ANTIQUITY* for September (p. 313) Dr Th. H. Gaster mentions the discovery of the word *AWARI* (=field) in the non-Semitic texts of Ras Shamra. This word was already known from Horrian texts, and is regarded as of Luvian origin by Dr Emil Forrer, who long ago pointed out that it explained convincingly the name *Αῶρις* (*AWARIS*) of the Hyksos settlement on the eastern margin of the Nile delta. In this case the name is well applied to a 'camp' (Latin *campus*=field), such as that at *TELL YAHUDIYEH* excavated by Sir Flinders Petrie and identified by him with Awaris.¹

The Hyksos are generally supposed to have been Semitic invaders of Egypt who came from the eastern deserts and steppes. It is quite likely that such nomads should have had to use a Luvian, Horrian (Mitannian) loan-word for a fortified permanent settlement. It is, indeed, well-known that the Hebrew word 'IR for a town or city is such a loan-word; *IRU* or 'IR being the Sumerian *URU* (=city) pronounced *üru* with the mortified vowel *ü*, as in *München*, Bavarian *Minka*, for *Munich*. This word occurs in the compound *IRU-SHALEM* which may mean either 'city of the god Shalem', a divinity found in the Ras Shamra texts mentioned by Dr Gaster (p. 311), or 'city of Peace', i.e. 'city of asylum' (*ἄσυλον*) or possibly both.

Dr Forrer was also the first to recognize *QARTH*, the other Hebrew and Phenician word for 'town', as another such loan-word. It occurs as the name of the celebrated Phenician colony *QARTH ḤADASHA* (=Carthage²), meaning New Town, and differentiating it from 'ATIQAH (=Utica), meaning 'the Old One'. It appears to be derived from the Indo-European language of the Philistine settlers on the Phenician coast or from that of the Hittite invaders of Northern Syria. It is obviously identical with the 'Hittite' i.e. Nashili word *GURT-ASH* in

¹ British School of Archaeology in Egypt, 12th year, 1906. *Hyksos and Israelite Cities*, by W. M. Flinders Petrie, pp. 1-27.

² There was also another Carthage, in Cyprus; it is mentioned, as *KAR-TI-ḤA-DA-ASH-TI* in a cuneiform inscription of Assurbanipal; see Oberhummer article 'Kypros', Pauly-Wissowa, *Realenc.*, XII, col. 102, line 9. Incidentally the Roman form *CARTHAGO* for *QARTH ḤADASHAH* proves that the Latin *G* was pronounced as a palatalised *ğ*, just as it shows that the Phenicians of the West pronounced the long *ā* (*qāmes*)—like the modern Ashkenazi—as an *ā* or *ō*.

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SHARRAZISH GURTASH, 'Upper Town'³ or 'Hill Top Town' (Greek Ἀκρόπολις). We may also compare the place-names Γύρτων, Γυρτώνη (GURTON, GURTONI) in Thessaly, Κυρτώνη (KURTONI) in Boeotia, CORTONA in Etruria⁴, possibly CORD-OBA in Andalusia⁵; the Slavonic GRAD, as in BELGRAD, the White City, NOWIGRAD, the New Town, STARIGRAD, the Old Town; Russian GORODŭ, town, as in NOVGOROD, the New Town, GORODISHČE, walled village⁶; Lithuanian GARDAS, enclosure; Gothic GARDS; German GARTEN, as in Stuttgart, Mömpelgart, Stargard (from Slav. Starigrad); English GARDEN, Gaelic GORT (diminutive GORTAN), a field, garden; French JARDIN, Old French GARDIN with which may be compared Lake Garda and Gardone in Upper Italy (Gallia Cisalpina); Latin HORTUS; Low Latin CORTIS, whence English COURT, French COUR. In ancient Crete, from which island the Philistines came to the coast of Palestine⁷, we find a GORT-YN (with which GORD-ION in Phrygia may be compared) and a KAIRAT-OS, the old name of Cnossos; with the last compare the Hebrew QARIATH (which became QARIAH when the old feminine ending -ATH was worn down to -AH), and the name PHONO-KERATOS which was the Greek transcription of the Iranian VANA-GERD.

The dictionary of Hesychios has a gloss—Κέρτα. Πόλις ὑπὸ Ἀρμενίων: 'Kerta, a Town (is thus called) by the Armenians'. Strabo (XI, 7, 26, p. 508) mentions a town Karta in Hyrcania, with which may be compared the place-names Χαρα-χάρτα, Βατρα-χάρτα, Πασα-κάρτα, Τυγρανο-κάρτα (modern Tigranskert), Ἀρταγι-γάρτα (modern Ardashi-Gert), Δαδο-κέρτα, Φωνο-κέρτος (modern Vana-gerd), Melaz-gerd, Manawas-gerd, preserving the old Chaldic royal name of MENUWAS, Greek MINYAS

³ *Keilinschriften aus Boghazkoej*, IV, 4; II, 61; E. O. Forrer, *Forschungen*, II, 46; A. Goetze, *Mitt. d. Vorderasiat. Ges.* 1936, 34, 2, p. 66, quoting *Keilinschriften aus Boghazkoej* V, 6; III, 33.

⁴ Charles Autran, *Introduction à l'étude critique du nom propre Grec* (Paris, Geuthner), pp. 204, 464.

⁵ See below p. 452, note 15 on KERK-OBA in Arcadia, CRAC-OW in Poland and p. 454 on the meaning of the -OVA ending.

⁶ This refers to the so-called 'fluchtburg' or refuge-camps, as they were formerly regarded; it is now known that they were simply villages, permanently inhabited, surrounded by a protecting wall. See Schrader-Nehring, *Reallexikon der Indogermanischen Altertumskunde*, 2nd edn., II (Berlin, 1929) 434, plates xcviii ff.

⁷ GENESIS, 10, 14 = I CHRONICLES, I, 12. Transpose: 'and Pathrusim and Casluhim and Caphtorim, out of whom came PHILISTIM'. CAPHTORIM, cuneiform KA-AP-TA-RA, hieroglyphic KPTR is Crete, the hitherto unidentified enigmatic Kasluhim are simply 'waste lands' (cuneiform KISLAH *Mitt. d. Vorderas. Ges.* 34, I, 1930, p. 82, line 35).

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(Μινύφας) and MINOS (*Μίνφως); Γίλι-γέρδα, Δασδα-γέρδα (modern DIS-CARTA); VAI-KERETA; SHAMIRA-GERD, the Town of SEMIRAMIS; EROWANS-GARD.⁸

Another such loan-word is the Aramaean (Syrian) word KĒRAK for a fortress, as in the Crusader castle Crac des Chevaliers,⁹ or a fortified citadel; KARKA, a town, principal town or metropolis is another such. These words must be related to the Egyptian GRG.T, Coptic KERKE, settlement,¹⁰ whence came the denominative verb GRG, to found, take possession of, GRG PR, to found a house, a family, settle down as a married man, marry; and the place-names GIRGE, GIRGAH, near Abydos, in Upper Egypt; GERG-IS, GERGI-NA, GERG-ITHA near Lampsakos and the region GERG-ITHIA in the Troad, all of them authoritatively¹¹ stated to have been settled by Cypriots. Compare also the name GERG-ITHES, townsmen, applied to the lower classes, that is, to the native population of Miletus as distinct from the aristocratic Ionian invaders; and the GIRGASH-ITES or GIRGAS-ITES of Palestine¹²; also the GERG-ITHI-ON near Larissa in the neighbourhood of the Aeolian Kyme, and the town GERGIS on the coast of Libya (the modern ZARZIS or DJERDJIS); also the 'Hittite' place-name KARK-EMISH, ancient GARG-AMISH, meaning township or possibly metropolis (mother-town)¹³ and formed like the name of the goddess ARTEMIS which can now be seen to be derived from the

⁸ See Haxthausen, *Transcaucasien*, II, 36; Brunnhofer, *Arische Urzeit*, 14.

⁹ Described in an early essay by T. E. Lawrence, posthumous edition (Cockerell Press 1936). See T. S. R. Boase, *Journal of the Warburg Institute* 1939, II, 16, note 2.

¹⁰ A list of Egyptian place-names compounded with the element KERKE will be found in Grenfell and Hunt's *Tebtunis Papyrus*, I, 383.

¹¹ See Pape-Benseler's *Dict. of Greek Proper Names*, and the respective articles in Pauly-Wissowa's *Realenc. d. Klass. Altertums*, giving authorities for those place-names here quoted without such.

¹² GENESIS X, 16: XV, 21; DEUTERONOMY, VII, 1; JOSHUA III, 10 and XXIV, 11; I CHRONICLES I, 14.

¹³ -AMISH, -EMISH might be composed of the well-known -ISH suffix and the widespread baby-language gloss ('Lallwort') 'AM, 'AMMA, 'Αμμάς. In Biblical Hebrew 'EM, 'mother' is actually used of a 'mother'-town. The compound KARK-EM-ISH, GARG-AM-ISH = 'Town maternal' like CITTÀ MADRE, CITÉ MÈRE and not like our 'mother-town'. Greek μητρόπολις would be formed like the common place-name 'Αστυ-παλαία (instead of Παλαιό-πολις) the un-Hellenic, i.e. pre-Hellenic construction of which has long been noticed, e.g. by August Fick in his *Hattiden und Danubier* and *Vorgriechische Ortsnamen*. On the face of it GARG-AMISH might be compared to PERG-AM-OS, PERG-AM-ON—PERG being evidently πύργος, BURGH, BROCH, etc.—but PERGAMOS (the son of Neoptolemos) might stand for *PERGAMUWAS, PRIAMOS for *PRIAMUWAS as PYRAMOS is PURANDA-MUVAS according to E. Forrer.

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Horrian (or Mitannian) word ARTE, city¹⁴ (ANTIQUITY, Gaster, p. 313). Artemis was 'she of the city', the Πολιάς, having charge thereof (Ephesus, Priene, Brauron, etc.) or possibly the 'Metropolitan' goddess worshipped by all the colonies a 'mother-city' had sent out. Other examples are the township Κέρκ-οβα in Achaia, i.e. KERK-OVA with the suffix -OVA as frequently occurring in place-names in the Hittite tablets of BOGHAZKÖJ¹⁵ as it does on the map of modern Poland, and the Slavonic (Polish) fortress Cracow, Krak-uv.²²

In the light of present knowledge it would appear that KKK, like QARTH, is an early Indo-European word—akin to the Latin *circus* and *circa*, around; the Greek κίρκος, κρίκος, a hook, and the Albanian (=Old Illyrian) KARK, a curve or curvature—denoting originally the circular walled towns characteristic of Syria¹⁶ and the ancient East

¹⁴ Sir Charles Low compares the Turkish word ORDU, camp, Persian ōrdū, court, camp, horde of Tartars, whence is derived the term URDU ZABAN, camp language, to describe the Persianized Hindostani (written in Arabo-Persian characters) used by the educated Moslems of India. This is a *lingua franca* which spread from the Moghul 'camp' or 'court' of Delhi. The English and German words 'horde', describing a savage 'tribe' (originally of Mongols or Tartars) are well known, and are generally recognized as loan-words from Turco-Tartar ōrdū. Comparison with the Horrite ARTE, city, is all the more legitimate since Dr Emil Forrer is inclined to consider the Horrians as hailing from Central Asia. On the other hand we find at Sparta a goddess ORTH-IA, whose certainly pre-Hellenic name has nothing to do with ὀρθός 'upright' since there is no evidence that she was ever worshipped in the shape of an upright pillar. She is identified with ART-EMIS, a fact which is easily understandable if both ORTH-IA and ART-EMIS mean 'she of the city', Πολιάς [θεά]=[the goddess] of the city, or 'of the Metropolis' and if in the various dialects of the 'Horrian' language an ARTE stood beside an ORTHE, both meaning literally 'camp, court'. It is remarkable that we find in German—and only in the recent high German—the words ORT, ORTSCHAFT, town, township, which cannot be derived (Kluge's *Etymologisches Wörterbuch*, 9th edition 1921, p. 336) from ancient high German *ort*, Anglo-Saxon, Old Saxon, Middle-English *ord* 'point' 'corner', but must be a Tartar loan-word like *Horde*. Then there is pasture, grass [land] or grass [plot], grazing: Latin HORTUS, Italian ORTO, garden, alongside with middle Latin CORTIS, CURTIS, Rumanian CURTEA, French COUR, English COURT, O. Engl. GEARD, hedged inclosure, English YARD, an inclosed place or forecourt of a building. One wonders, therefore, whether the Horrian ARTE, Turkish ōrdū, Persian ōrdū, Spartan ORTH-IA might not somehow be connected with the Indo-European group GURT-ASH, GORD-ION, QARTH, KERT, GERD. Being nomads like the Semites, the Turco-Tartars and Mongols might have borrowed the Iranian word ōrdū.

¹⁵ E.g. ANK-UWA, ANS-UWA, HASH-UWA, HIND-UWA (obviously connected with the river Indos) in Caria; ISH-UWA (obviously connected with the river Issus, site of Alexander's famous victory), LULL-UWA, SAPIN-UWA, TUWAN-UWA (=Greek TYANA), UGAB-UWA, ZINZIK-UWA.

¹⁶ See e.g. the plan of Sam'al in Northern Syria: Ebert, *Reall. d. Vorgesch.* 1, plate 102.

generally. A good example is Erbil (Arbela) in Kurdistan, a modern circular walled town standing on a debris-mound of immemorial antiquity (ANTIQUITY, 1936, x, 136, plate 1). On the early Babylonian map found at Nuzi (c. 2500 B.C., ANTIQUITY, June 1936, x, 226, pl. VIII) the three cities on it are represented 'by circles with their names written within the circles' just as towns are represented by circles or circular dots on the most modern maps. The early Egyptian hieroglyph , added as the determinative after the proper names of all inhabited places, suggests a circular walled city. The Latin *urbs* 'city' is probably nothing but a graphic variant of *orbis* 'circle'.

At first it might seem surprising to find an Indo-European, presumably Luvian, loan-word in early Egyptian texts and amongst the names of places on the Libyan coast.¹⁷ But I remember that Dr Emil Forrer has called my attention to the striking correspondence between the common Egyptian verb SDM, to hear, and the synonymous Luvian word ISHDUM-ASHSHUWAR.¹⁸ There are other points of contact between the old Egyptian and Indo-European languages. The Egyptian *irp*, the vine, identical with the Greek *ἔρπ-ις* occurring in a fragment of the poet Hipponax is plainly an Indo-European word meaning 'creeper' (*ἔρπειν*, to creep, Latin *serpo*, whence *serpens*, serpent). Probably the word entered Egypt with the plant, which is not indigenous in the Nile valley. On the other hand in Thrace it grows wild to this day.

A still more important instance is the Egyptian word *iw*, island or far distant sea-coast. With it may be compared **ios*, the proper name of an island in the Aegean;¹⁹ also the Phoenician and Hebrew *ʾis*, *ʾij*, corresponding to the Greek *aia*, originally *aifā*, as in *ʾAχ-aia* (**ʾAχ-aifā*), the cuneiform AKH-IJA, AKHI-JAWA or AKHI-JAUWA. Compare too the suffix -AVIA in the composite names of the countries Maz-ovia, Mor-avia, Mold-avia, Scandin-avia, Austr-avia, Act-avia and the unidentified AP-PA-VI-A (read *āp-avia*) in the Boghazköj tablets.


The old name for the Peloponnese was *ʾAπ-ia*. The Achaeans

¹⁷ The Greek *Λιβύη*, Libya, is obviously a transcription of a native LIB-UWA to which cp. Lieb-au, Ljep-aja on the Baltic coast. As to the meaning of the suffix -UWA, -OWA see p. 454. On the Libyan coast are the Kerk-ennah islands, a name which may well be synonymous with that of the Greek *Cyclades* (*κυκλάδες* from *κύκλος*).

¹⁸ ASHSHUWAR is the ending of the Luvian infinitive; the initial I may well be due to the exigencies of the syllabic cuneiform writing, which could not render a verb SHDUM in any other way; or it may be the 'anaptyctic' vowel, facilitating the pronunciation of the two consonants following each other; or it may be a case like *εἰς τὴν πόλιν* < Istanbul < Stambul.

¹⁹ Cp. *Oeland* i.e. the 'island' on the Swedish coast.

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(Ἀχαιοί) of the Greek texts appear as 'AQI-AVA-SHA, with hieroglyphic  (=q), in the inscriptions of Merneptah (c. 1220 B.C.). The Latin *aqua* (water), the Illyrian river-name AQUILIS (in Istria) and that of the watering place AQUINCUM (in Pannonia) with its hot springs (-cum to Greek καῦμα) correspond to the Greek river-names ACHĒ-LŌOS, ACHĒ-RŌN (near Dodona), ACHI-ROĒ in the Argolid, and to the old Teutonic word AHA, German ACHE, river, occurring in many compound place-names, such as UNTER-ACH, ACHEN-SEE. It would therefore seem quite legitimate to compare the cuneiform API-AWA and the corresponding Greek name AP-IA for the PELOPON-NĒSOS (Pelopian peninsula) with the early Indian -AP, Avestic -APAM,²⁰ water. Compare also the Thessalian river API-DANOS and the Arcadian river API-DON. The second part of these last two river-names is identical with the Scythian word DAN, Avestic DAN, Ossetic DŌN, river, occurring in such names as DAN-AP-RIS (the Dniepr), DAN-ASTRIS (the Dniestr), ERI-DANOS, RHO-DAN-OS 'the Rhone' and the rivers Don in Yorkshire, Scotland and Russia.

If that is correct, the -AWA, -AIWA, -IA in AKHI-AWA, AKH-AIA, API-AWA, APIA, originally *aiFa watered land or earth, the -AVIA = *AGVIA in SCANDIN-AVIA, AUSTR-AVIA, ACT-AVIA (cp. ἄκτῃ, coast) MOR-AVIA (from 'mor' = a moor), the German AUE, -AU, in place-names such as REICHEN-AU, ILMEN-AU, Dutch and German EI in the word EI-LAND, Icelandic EY-LAND, the Scandinavian word ö in the word ÖLAND, island,²¹ and in place-names such as MALM-ö, TROMS-ö, etc; the OWA in the Arcadian Κέρκ-οβα, the Celtiberic CORD-Oba, the Lithuanian LJET-UVA; the -OW or -UW in the Slavonic CRAC-OW, KRAK-UW,²²

²⁰ This Aryan AP[AM] appears as -AB in DO-AB, land of 'two-rivers', PUNJ-AB, land of 'five rivers'; it corresponds to the Latin AQUA, Illyrian AQUILIS, AQUINCUM, and the Teutonic AKHA as the word PIDPID in the unidentified language discovered by Emil Forrer (Die Pidpid-Sprache, Forschungen II) on a cuneiform tablet from Boghazkoej, with the Latin QUICQUID and the Nashili KUISHKUISH (Latin QUISQUIS), or the Latin LUPUS with the Greek λύκος. Similar correlations are well known to exist between the p-languages and the q-languages of the Celtic group.

²¹ The 's' in 'island' (ME. iland, OE. ig-land) is well-known to be due to a confusion with isle, derived from the French *isle*, Italian *isola*, Latin *insula*, the diminutive form of the word *ns surviving in Gaelic: YNIS (Welsh), INNIS (Gaedhelic), ENNIS (Irish), INCH (Scottish; cp. for these words Isaac Taylor, *Words and Places*, 1896, p. 329) and in the Greek νῆσος, νῆσος (whence *nash-ili*, the language of the Νησιώται, the Indo-European conquerors entering Asia Minor by way of the 'islands bridge', the Indo-European language of the Boghazkoej tablets). The g in Anglo-Saxon IGLAND shows that AVIA is derived from an earlier AGVIA.

²² Popularly derived from the name of a mythic founder of the town, the giant Krak.

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would be nothing else than the frequent (Luvian) -OVA, *i.e.* -AUWA in the Boghazköj place-names such as HIND-UWA²³ = FLUSS-AUE, river marshland, just as DAN-UVIUS, DAN-UBIUS, DON-AU would be the DAN-UV-IUS (fluvius), the stream of the alluvial river-land.

The only genuine Semitic word for 'town' is found in the Accadian language of the Assyrians and Babylonians who say ALU for 'city'. It was long ago correctly explained by A. H. Keane²⁴ as the cuneiform transcription of 'AHLU, 'AHALU, Hebrew 'OHEL, Arabic 'AHL^{un}, a tent. Nomads would of course call any spot where they had camped a 'settlement', and as soon as they had captured fortified and permanent settlements, they would adopt the local native (foreign) word in each case to describe them.

ROBERT EISLER.

ROMAN CAMP-SITE NEAR CASTOR (PLATES I-II)

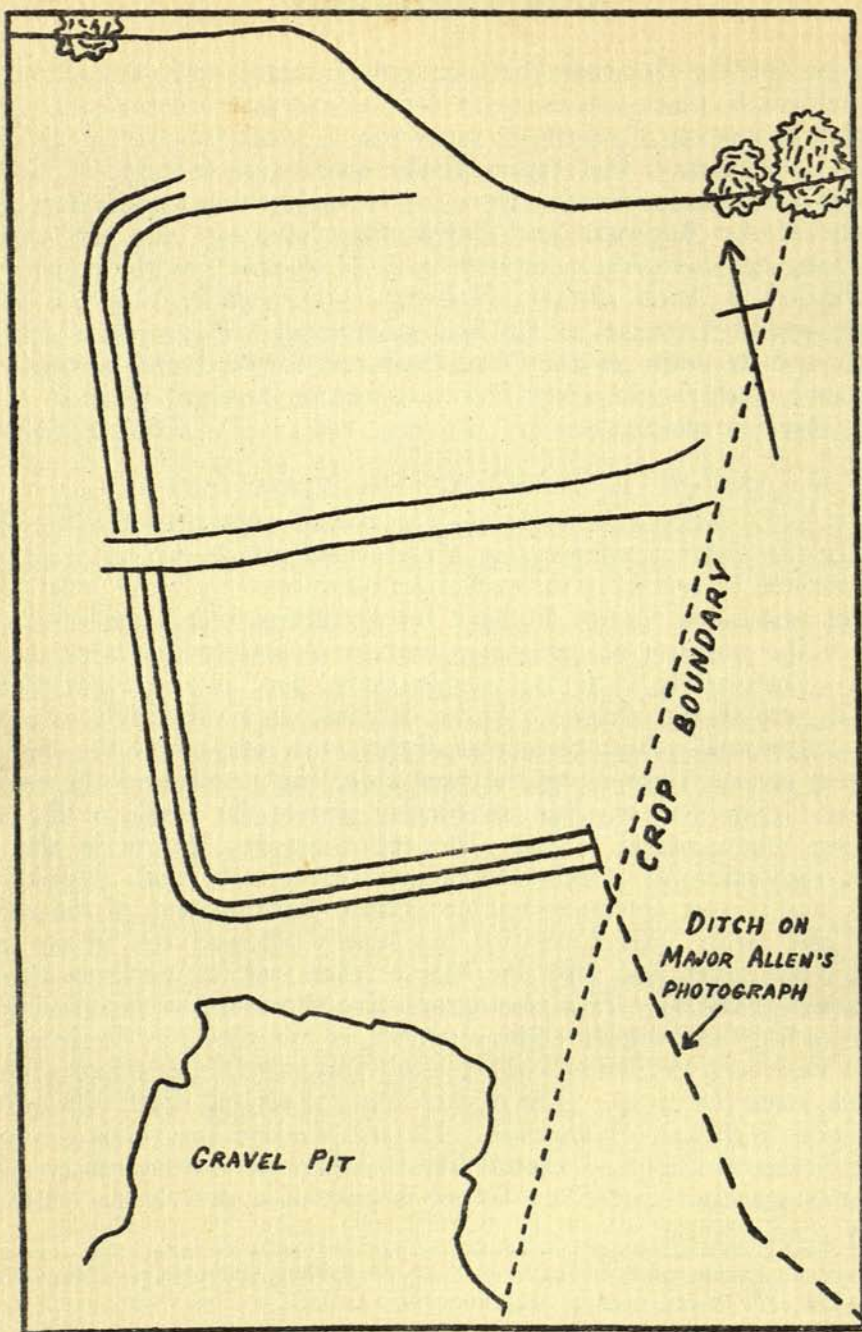
Mr I. D. MARGARY writes:—The partial exposure of this camp shown in the plate¹ accompanying Mr Hawkes' article can, after all, be supplemented by one from the earlier set mentioned by him.² (PLATE I). This set was taken for me in 1931 by Aerofilms, but I was not then aware of the position of this camp and so its indications were overlooked. Fortunately, it is the western side, poor in the other plate, which is here shown clearly. Three ditches can be distinctly traced, almost continuously, from the centre of the south side, round the curved southwest corner, right along the west side, and just round the curve of the northwest corner, thus completing almost the whole outline of the camp when added to the other photograph. There is also a distinct indication of a centrally placed east-west road, edged by ditches, and a west entrance, but no definite continuation of the road beyond the camp. Mr Crawford has kindly allowed me to see an oblique view taken last July by Major Allen, which confirms these indications (PLATE II). This photograph also shows one of the roadside ditches leading out from the south entrance, as recorded by Mr Hawkes, but it is here seen to turn at a sharp angle towards Billing Brook about 130 yards from the camp. The existence of the second ditch cannot be determined from this photograph. Having studied the roads joining Ermine Street at Castor, I should like to add some comments on Mr Hawkes' suggestion that King Street is part of a pre-Ermine Street military road.

²³ To these Asianic names ending in -AWA, cp. the modern LIET-UWA, *i.e.* Lithu-ania.

²⁴ *The World's Peoples*, 1908, p. 324.

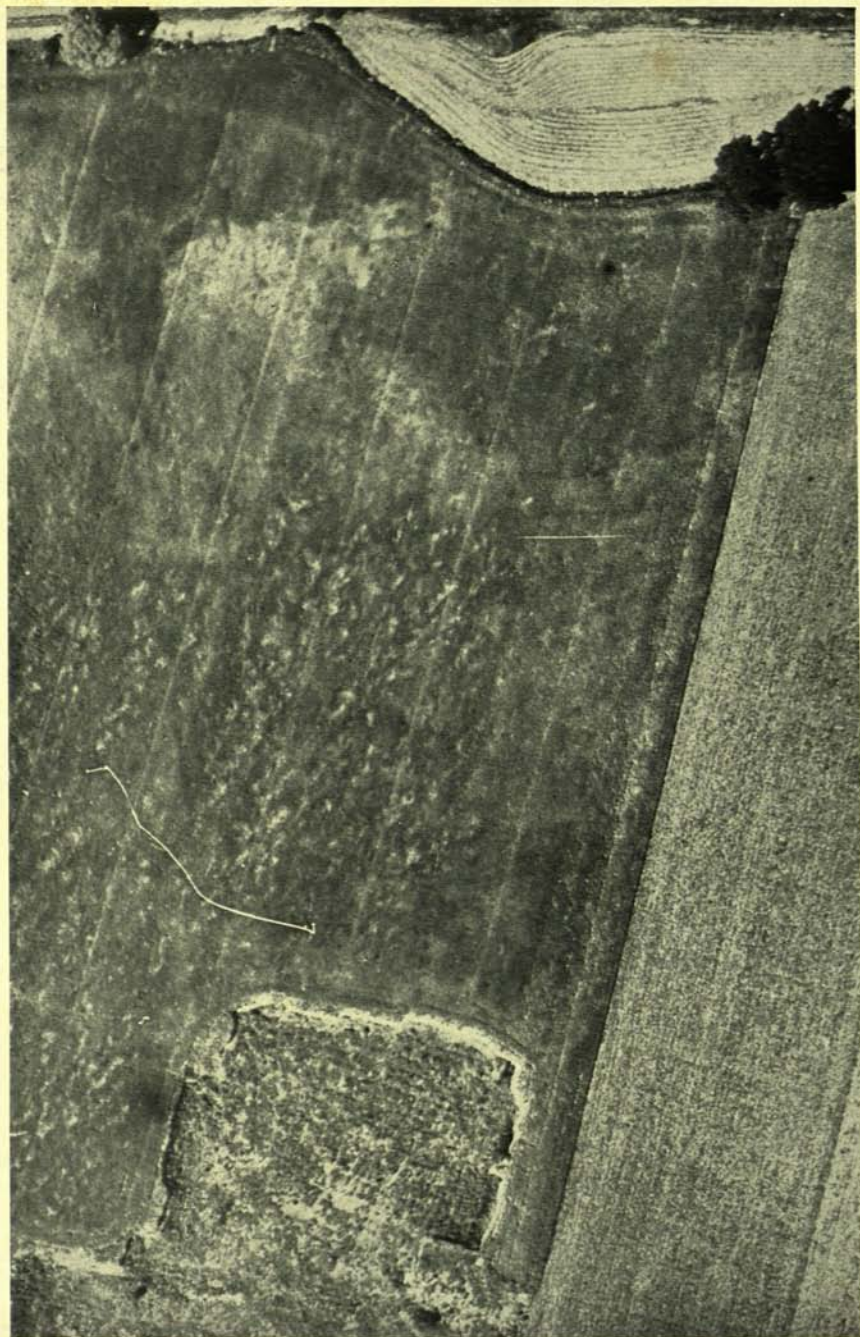
¹ *ANTIQUITY*, XIII, facing p. 184.

² p. 185, footnote.



SKETCH-PLAN OF THE APPARENT AIR-PHOTOGRAPH EVIDENCE FOR THE CASTOR CAMP (cf. PLATE 1)

PLATE I



THE WESTERN HALF OF THE ROMAN CAMP-SITE AT CASTOR (see p. 455)
Ph. Aerofilms Ltd.

PLATE II



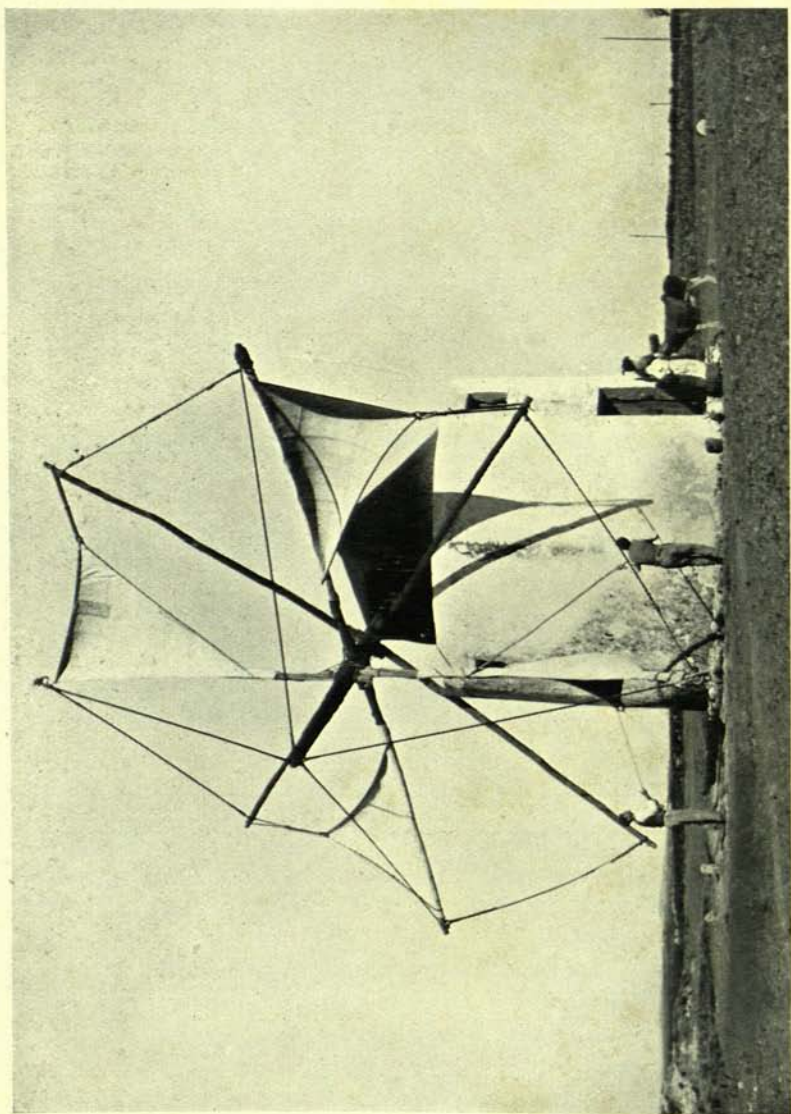
THE ROMAN CAMP-SITE AT CASTOR (see p. 455)
Air-photo, Major G. W. G. Allen

PLATE III



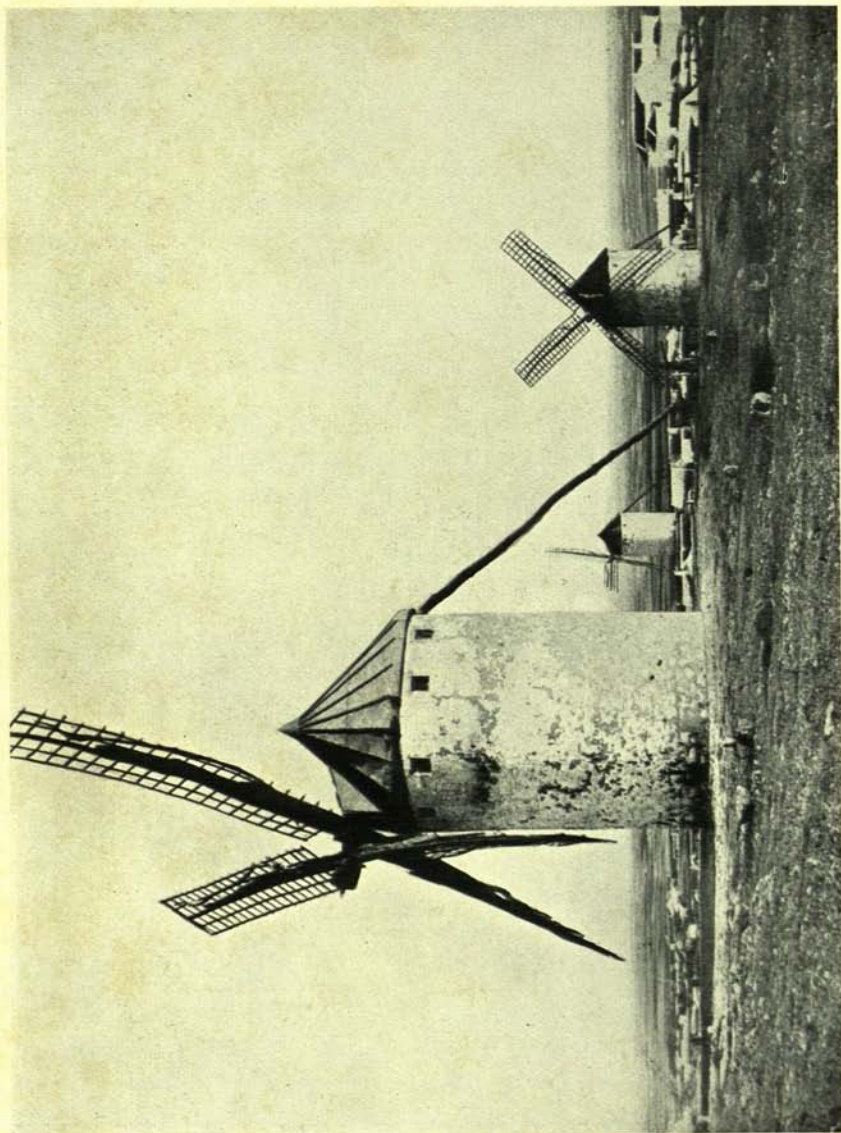
WINDMILLS IN CRETE (*see p. 458*)
after Marlin Hurlimann

PLATE IV



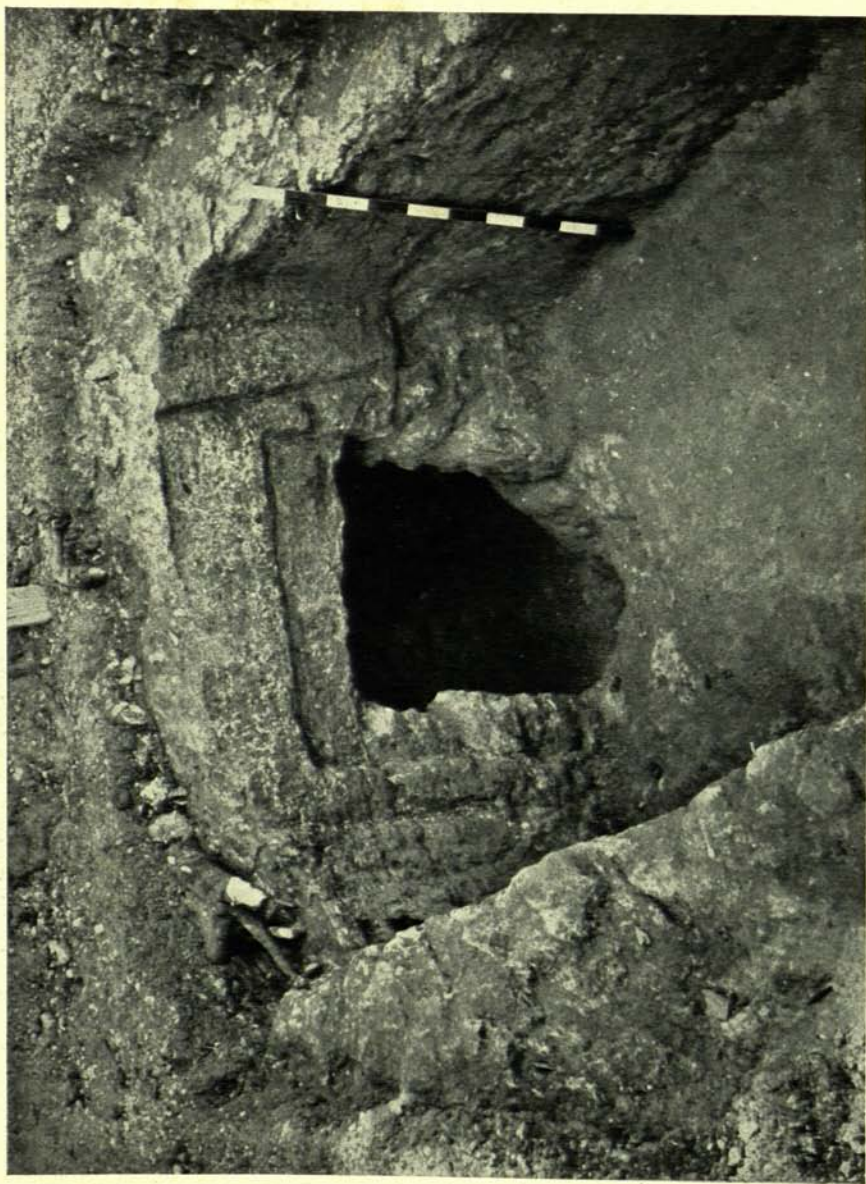
WINDMILL BETWEEN LISBON AND CINTRA (see p. 459)

PLATE V



DON QUIXOTE'S WINDMILLS AT LA MANCHA (*see* p. 460)

PLATE VI



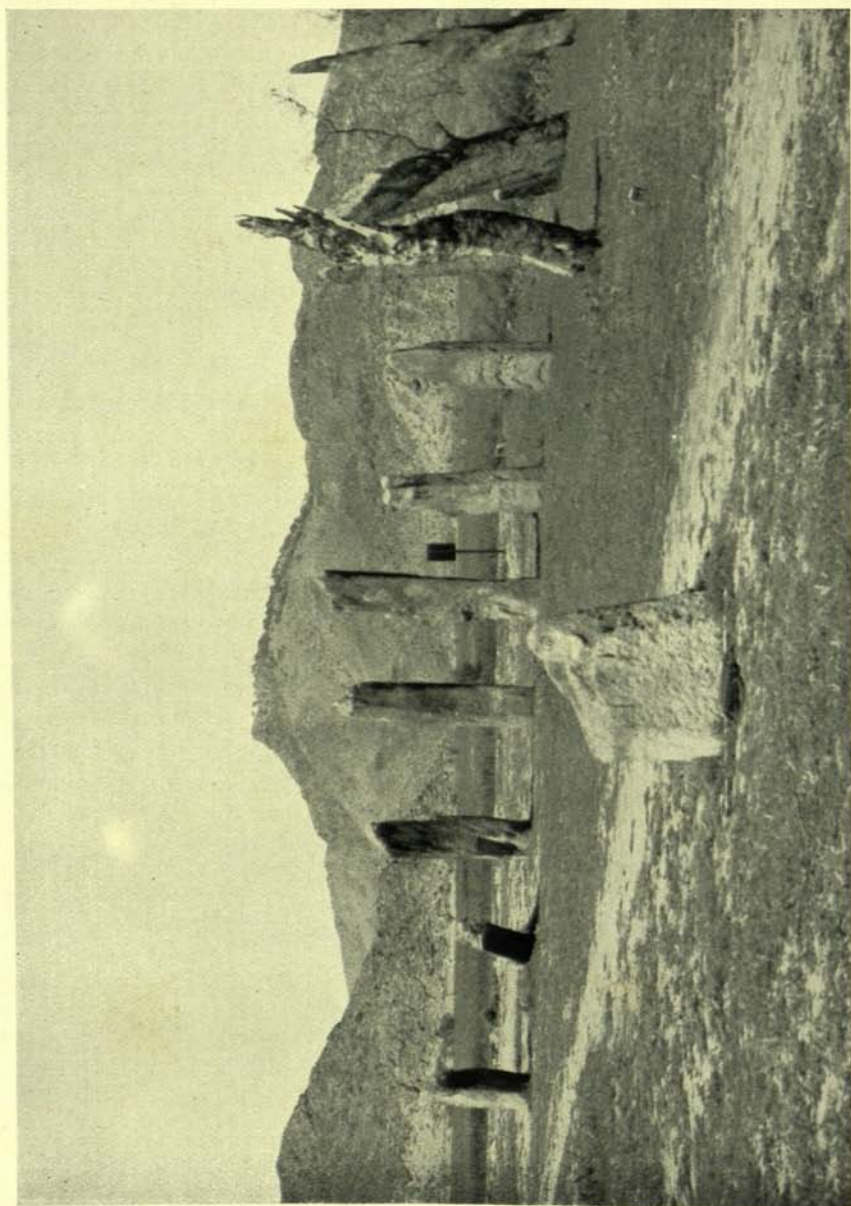
TOMB 116, VOUNOUS, CYPRUS: THE FACADE AFTER THE REMOVAL OF THE ENTRANCE STONE (see p. 461)

PLATE VII



INCISED FAÇADE AT LAPATSA, KARMİ, CYPRUS (see p. 463)

PLATE VIII



STONE CIRCLE, ASOTA, INDIA (see p. 464)
East stone in foreground; the West leaning stone on left, beyond
Ph. D. H. Gordon

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It is suggested that the *general* direction of King Street towards Bourn starts from this camp site rather than from the Roman town site called 'the Castles'.³ The matter is somewhat confused by what appears to be a definite, though slight, change of alignment at the north end of Ailsworth Heath. The *main* alignment north of that point, if carried southwards, meets Ermine Street exactly at the Nene bridge, and would pass just east of the camp, thus suggesting that the crossing was already there when King Street was planned. But the agger can be clearly traced along the west side of Ailsworth Heath for $1\frac{1}{2}$ miles, till it is lost near Upton, and this minor alignment joins Ermine Street over 500 yards north of the river, thus saving that length of road-building if Ermine Street was already there. The existence of the agger on the Heath seems good evidence that this line was followed, but, again, it is aligned 160 yards west of the camp, not directly on it. Neither line can strictly be said to be laid on the camp. If Ermine Street were already there, the whole planning of King Street in this area is quite explicable as shown above, otherwise it seems meaningless.

I agree that the evidence of the legionary tile from Hilly Wood indicates an early date for King Street, but it does not prove that Ermine Street was not early too. The alignment of this road seems fundamental to the Roman planning of the district, and no evidence has yet appeared for any pre-Ermine Street such as Hawkes' argument requires. Until this is forthcoming it seems premature to assume that the short length of road in the south entrance of the camp, not aligned with any other Roman road, is anything else than a connecting link with the known Ermine Street.

The observations in my paper⁴ as to the probable *relative* dating of Ermine Street, King Street and the Fen Road (my Road II) still hold good, save that King Street is now shown to be definitely early. Mr Hawkes has, I think, misunderstood me when he says I contended that Road II joined King Street and *not* Ermine Street;⁵ it may well have done both, and I did not intend to imply more than that Road II was a later by-road which connects with the accepted road system at King Street.

Mr HAWKES comments:—'It is of course quite possible that Mr Margary is right about King Street. My suggestion of its priority was simply a suggestion, which remains to be proved or disproved. The total disappearance of its line between the Nene and the south end of

³ p. 183.

⁴ *Antiq. Journ.*, xv, 114-5, 118.

⁵ *ANTIQUITY*, xiii, p. 185, footnote.

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Ailsworth Heath makes it impossible to say anything more at present. But I think it is always worth remembering that 'what seems fundamental' in such a piece of Roman road-planning is not necessarily the earliest feature in time, but may have been designed to supersede less satisfactory earlier arrangements belonging to an initial military period. With regard to Mr Margary's Road II, I regret my misunderstanding, and quite agree with his observation. It is in every way most fortunate that his photograph, supported by Major Allen's new one, does so much to supplement that published with my article. May the time for following up these reconnaissances by excavation not be far distant'!

MEDITERRANEAN WINDMILLS (PLATES III-V)

The Islands of the Aegean are full of windmills of a type very different from those of England and the north of Europe. Many of them are ruinous, but a considerable number are still working.

In other parts of the Mediterranean large numbers of ruined towers exactly like those of the windmills of the Aegean Islands are to be seen; but as a rule it is not possible to say that they were once windmills, though in a few cases the presence of mill-stones on the ground outside their doors shows their origin. The only active mill of the kind I have seen outside the Aegean is in Portugal.

The peculiarity of these windmills is that their sails are set like those of a boat, and not spread on a lattice framework as is the case when canvas is employed for mill sails in England. The mills are many-armed. Some have eight arms, others ten or even twelve. The arms are slender round poles, and they are supported by stays of wire which start from the end of a long pole that sticks out in the axis of rotation. The arms are also tied to one another with wire or iron chains. The sails are triangular, the base being toward the circumference and the apex pointing to the centre. One edge is fixed to the pole and the opposite corner is held by a rope. When the mill is idle they are twisted round the poles; and when about to be used they are unwrapped to a greater or less extent according to the strength of the wind. All this was demonstrated to me by the miller of Patmos, who stopped his mill on purpose to show me how it worked, and started it again, turning the top to the wind, and showing me all the machinery.

These mills always occur in groups. Some are high up in the hills on the sky-line. But others are near the sea, often, as at Rhodes, set out along the harbour.

The towers of these mills never taper as ours do, but are invariably

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cylindrical. They vary only in height, and are very strongly built of stone.

Their works are, as one might suppose, primitive. There is scarcely any metal. The wheels and cogs, as in our own mills, are made of wood. Only the top turns round. It contains the backward prolongation of the great round beam through which the arms are thrust through mortices cut one in front of another. The beam is slightly inclined in the mills that I have examined, and it carries a great wooden wheel, along one side of whose rim the cogs are inserted. These cogs engage with teeth cut in the wall of an upright cylinder which is fixed to the steel axis of the upper mill-stone.

The method of turning the top of a mill to the wind differs in different countries. In some parts of France and Spain a long pole sticking out behind is employed. In England there is often a subsidiary sail which works on a ratchet on the top of the tower.

In Patmos, and presumably in the other Aegean Island, it is effected in a much simpler manner. On the top of the stone wall of the round tower there is a massive ring of hard wood from which project upwards pegs that reminded me of the thole pins of a sea rowing-boat. The conical cap of the mill is supported by a similar ring of hard wood. This ring is just a little larger than that which caps the tower. And it is set a trifle higher, so that its under surface is on a level with the upper surface of the latter. On its inner surface are a number of round holes. When the miller wants to turn the head of the mill to the wind, he just takes a long wooden pole, inserts the end into one of the holes, and using a peg as a fulcrum, levers the head of the mill round a few degrees at a time. This process was demonstrated to me by the miller of Patmos.

I have said that these mills are going out of use, and it is true that one often meets with ruined towers. But active windmills showing no sign of decay, and with their sails spinning round merrily to the wind, I have seen at Santorin, Cos, Kea and Rhodes, and I have photos of an active group at Chios. There is one at Gallipoli, near Cape Hellas, which looks like a modern one, and I have seen windmills of the kind which appeared to be in working order between Lisbon and Cintra.

At Cos, when I was there in 1929, I saw seven windmills stretched out in a row on the sandy spit which runs out to the west of the town. One, it is true, was a ruin, but four were in full sail, while the other two had sails wrapped round their arms, and appeared to be in working order.

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At Rhodes the windmills along the harbour mole are ruinous, but there is still one in the town in working order.

I remember my first sight of these eastern windmills. It was off Kea (or Zea), an island south of Attica. From the ship's deck I could see, a mile or two away, a fertile, rocky island. High up on the skyline was a row of ten or a dozen windmills; most of them were ruined and without sails, but one was spinning round rapidly. I could see it had many arms, but whether ten or twelve I could not be sure. The slopes of the mountain were abundantly terraced, for the island is a fertile one, and about halfway up I could see a white town or village. Since then I have seen many of these windmills in islands that were once Greek. But elsewhere in the Mediterranean I believe they have gone out of use. I believe they were once common and, as I have said, they are still to be seen in active use near Lisbon. At Palma in Majorca there are two large groups of ruined towers which seemed to me like old windmills, but I could find no evidence of this, though I looked carefully for mill-stones. I believe there is a working mill of this kind in the interior of the island. At Tangier, on the hill at the back of the town, I found two ruined mill-towers, with their mill-stones lying before their doors. On the Asiatic coast of the Dardanelles I have seen similar towers.

There is an old picture, dated 1802, of a mill at Minorca. It has a rather tall cylindrical tower like an Aegean windmill, but its six arms carry sails like those we see in England. Nevertheless these arms are stayed with cords or wire radiating from an axial pole. I am therefore rather inclined to think that modern sails were put on an old mill during the English occupation.

In the *Geographical Magazine* for June 1935, p. 41, is a photograph of a mill at Iviza, with four lattice sails on which canvas is stretched as in some windmills in England. But the round tower is of stone, and the arms are supported by stays which start from an axial pole, just like those in the windmills of the Greek Islands.

The famous mills of Don Quixote at La Mancha have towers and tops like the ones I have been describing, but they all carry sails like those which we see at home. That they originally had sails like the mills near Lisbon or in the Aegean seems not improbable.

Lastly there is Daudet's windmill at Fontvieille where he wrote his tale, *Lettres de mon moulin*. A few years ago it was ruinous, but it has been restored, and now carries sails of modern type. What the old sails were like I do not know, but the tower is of the massive cylindrical pattern.

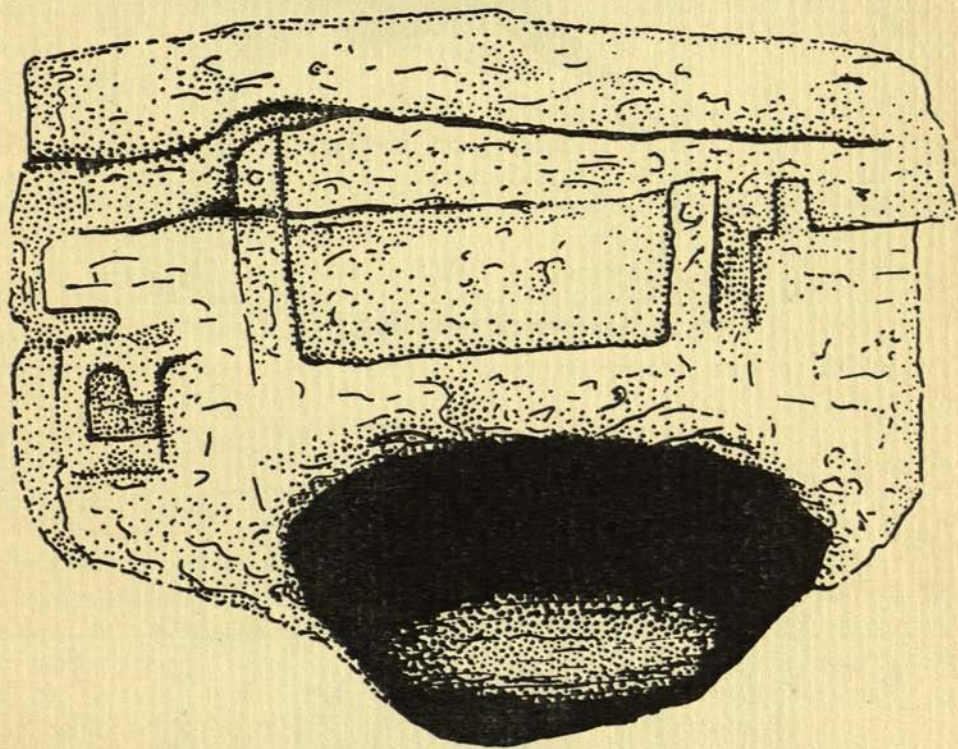
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Many modern wind-pumps based on the principle I have been describing are to be seen in Rhodes. The type of sail, therefore, which is set like that of a ship, is not considered inefficient, but is still used in modern constructions.

LOUIS COBBETT.

DECORATED TOMB FAÇADES, CYPRUS (PLATES VI-VII)

The recent excavations at Vounous¹ on the north coast of Cyprus, sponsored by the British School of Archaeology at Athens, revealed

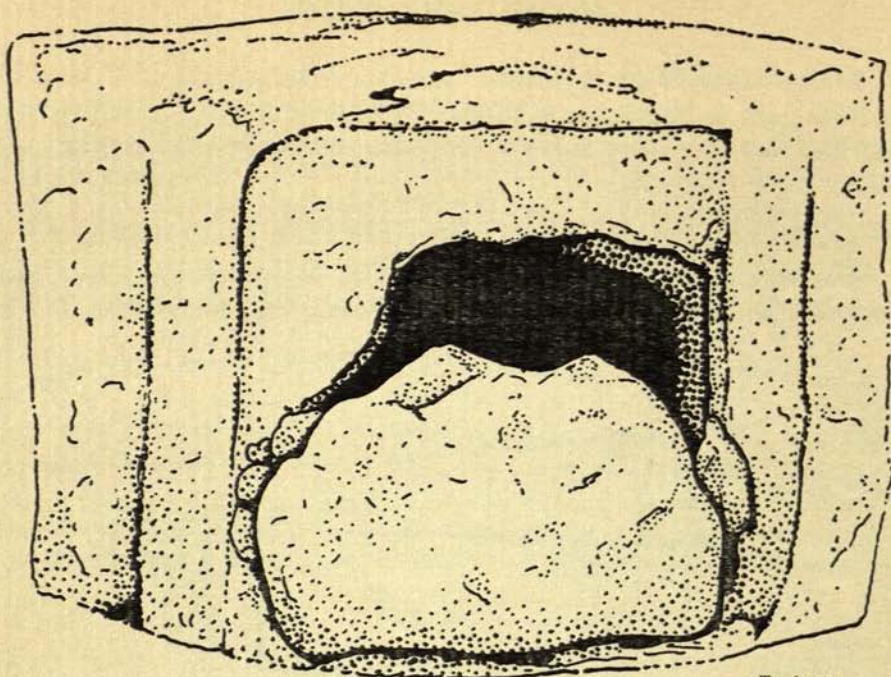


VOUNOUS, TOMB 117 ($\frac{1}{3}$)

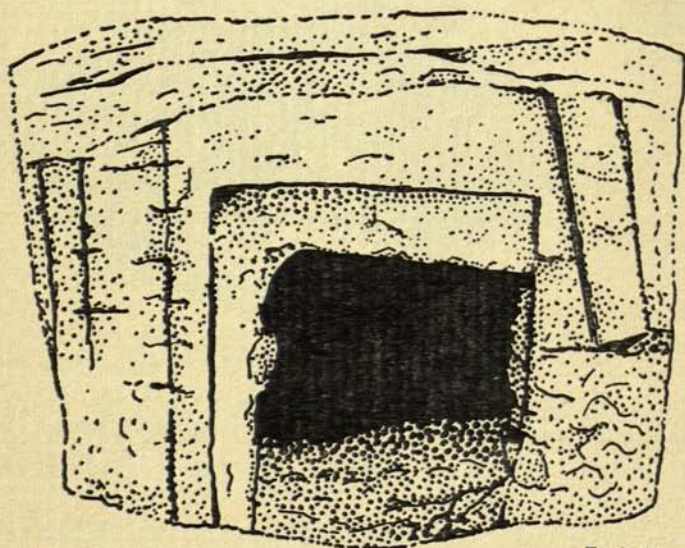
three tombs with carved façades; tombs 114, 116, and 117. The carving is carried out on the external face of the Chamber entrance, *i.e.* at the back of the Dromos or entrance passage. The drawings of the façades are reproduced here.

In these three cases the design appears to be architectural rather than decorative. Very little is known about Cypriot houses in the

¹ *ANTIQUITY*, September 1937, p. 356 and plate xi.



Tomb 114



Tomb 116

TOMBS AT VOUNOUS, CYPRUS ($\frac{1}{10}$)

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Early Bronze Age, but Dr Gjerstad has demonstrated that the form tended to include approximately rectangular rooms² arranged in an L shape round an enclosed courtyard. In this house at Alambra the rooms are not inter-communicating, and Dr Gjerstad has assumed with great probability the use of flat roofs as in modern Levantine houses. The walls were built of mud-brick on stone foundations, as are today the walls of village houses. This constructional form would have necessitated the use of timber uprights and lintels for door gaps, and the writer would suggest that the resultant appearance has been reproduced in the Vounous façade carvings, with the addition of a bolt slot on the left of Tomb 117.

All three tombs belong to Early Cypriot I, sometime before 2600 B.C. Tomb 116 is probably to be attributed to E.C. 1a, and 117 was probably excavated at about the same time, although reused in E.C. II-III. Tomb 114 had been looted, but produced sherds attributable to E.C. 1c at the latest.

A very different aspect is found at the nearby site of Lapatsa on the lands of Karmi village. A few tombs were looted during 1936-37, and one bears an incised chevron pattern on the upper façade, above the entrance-stone. This motif is purely decorative, and is well known on the pottery of the Early Cypriot period. The finds from this group of tombs, as now in the Cyprus Museum, belong to E.C. 1a-b. There are no pieces typical of E.C. II-III, so it is safe to assume that this tomb is approximately contemporary with the three Vounous examples.

The cornice cut to accommodate the stone closing the chamber-entrance should not be included in the class under consideration. It is rather commoner, being found at Lapithos in tombs 61, 64, 66, 67 and 69, according to Gjerstad.³ These tombs date from E.C. II to E.C. III. A similar feature has been recorded in relation to small cupboards cut in the Dromos wall for offerings.⁴ This feature belongs distinctly to the realm of tomb architecture.⁵

It is worth noting in this context the editorial notes in *ANTIQUITY*, March 1931, p. 1-4.

The drawings have been prepared for publication by Messrs A. West and Partners, from the field-drawings made by Mr J. S. Last.

J. R. STEWART.

² *Studies on Prehistoric Cyprus*, p. 20 ff.

³ *ibid.*, p. 70. ⁴ *ibid.*, p. 71.

⁵ Compare the similar feature in Sicily, Mallorca and the Dolmen de Mississipi. *Proceedings of the Prehistoric Society*, 1935, p. 112.

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MEGALITHIC STONES, ASOTA (PLATE VIII)

The Stone Circle of Asota in the Mardan District of the N.W.F.P. is situated close to the east side of the cart-track to the large village of Shewa, and level with the village of Asota. This cart track takes off from the 17½ milestone out of Mardan on the Mardan-Swabi road.

It is an almost unique monument in Northwest India, the only other example being the Megaliths of Burj Hama, illustrated in ANTIQUITY of June 1937. There is, however, not the least resemblance to connect the two; the stones of Burj Hama are huge, shapeless and of assorted sizes, while those of Asota are tall and narrow, and, there is little doubt, were originally much of a size. The plan of Burj Hama is irregular, if not incomprehensible; that of Asota an extremely regular circle.

Judging from the only segment in which every stone is standing, there must originally have been 32 stones. Of these, numbering the north stone 16, that one and a missing 32 are due north and south, and 8 and 24 due east and west. At least, taken by prismatic compass, the present position of stones 8 (leaning) and 24 (stump remaining) is a line roughly due east and west, and from 16 to a point at the centre of the circle due north and south. The circle has a diameter of 57 feet, measured from the inner faces of opposite stones at the point where they meet the ground. It is probable that originally all were of this height.

Fourteen of the stones are missing, including the whole of the southeast segment, 25 to 32. The distance between stones now standing is irregular, that between 4 and 5 being 2 feet 2 inches, and that between 9 and 10, 4 feet 4 inches. Some of the missing stones are built horizontally into a wall of a neighbouring grave-yard, and one, possibly no. 6, lies outside the circle to the west. This stone bears a number of lines on it at, roughly, intervals of three inches, some being a trifle more and others a bit less. On the far side of the stone there are a few similar lines even more irregularly spaced. They may have been executed at any time and with any form of tool. I examined them through a magnifying glass and they are roughly cut, but the stone is friable and it is quite impossible to say anything definite about them. As none of the other stones is cut or ornamented in any way, I fancy that these cuts are of relatively late date.

Just inside the grave-yard to the south there is a stump of stone of the same character as those forming the circle. It is covered with oil or ghi, and is probably of present-day religious significance, though

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part of a Muslim cemetery. It is 98 feet from the centre of the circle, and on a bearing of 204 magnetic. I think that it is a transplanted stone.

The circle is associated with a local legend of the usual 'people turned into stone' type. A raiding party is said to have surprised and ravished some women who were working in the fields. As the raiders made off, back over the neighbouring hills, the women called upon the Almighty to visit them with a judgment. Whether or not the Omniscient was aware that their lamentations were in excess of the genuine outrage to their virtuous feelings, he changed the protagonists impartially into stones; sundry boulders on the hill-side being indicated as the raiders, and the circle as the doubly ill-fated ladies.

D. H. GORDON.

TOMBS OF THEBES

We have received the following communication from Mr. N. de G. DAVIES :—

'I have been assured that you are likely to be sympathetic enough to give publicity to the deplorable occurrence—or rather recurrence—that I have to report. I have become convinced, by the failure of several appeals to the authorities concerned, that the enlistment of public opinion is the only recourse left to those who deplore the circumstances and are horrified by the prospect they present.

'The *Service des Antiquités* of Egypt, under whose protection the four hundred or so painted or sculptures tombs of Thebes lie, has recently begun to send native craftsmen, without European supervision, to repair, and even to restore, the painted tombs, beginning with several of exceptional beauty and importance, though they were in no real need of attention.

'Apparently some native authority thought that visitors would appreciate white-washed sepulchres, with walls made smooth and regular by filling up all inequalities with mud or plaster. One knows how difficult such work would be for the most judicious man of taste, and it may be judged what ruin would be wrought by men of primitive or superficial education. The result has been that several of the finest monuments of Thebes, tombs of quite unique attractiveness, have been reduced in a few weeks to wastes of glaring Plaster of Paris. The general beauty they retained, in spite of more than three millennia of neglect and injury, is entirely lost, together with many details of historic value. There is not the least assurance that this vandalism will cease. I have the statement of the Director of the *Service* that this action was taken without orders from him, without his cognizance, and to his own dismay; yet he has been unable to prevent a repetition of the scandal after the first case had been brought to his notice. The preservation of the tombs of Thebes, which are still in large measure uncopied and unedited, is the concern of the civilized world; yet I have found it impossible as yet to get any learned body to take action or even to publish the facts'.

Reviews

ARCHAEOLOGY AND SOCIETY. By GRAHAME CLARK. pp. XIV, 220, with 24 plates. Methuen, 1939. 7s 6d.

Dr Clark has already established an international reputation as a first-class excavator and field worker, a master of detailed research, a beautiful draughtsman and a successful organizer and editor. His new book reveals him also as a popular writer capable of expounding Prehistory to wider circles not only with erudition and lucidity, but also with a philosophical depth and a breadth of vision quite exceptional among archaeologists. His aim, set out with unusual modesty in the blurb, is 'to describe for the general reader how archaeologists go about their work'. Thanks to a lively and graceful style and a wealth of admirably chosen illustrations, the book succeeds brilliantly in explaining to the man in the street the aims of the prehistorian, the principles by which he hopes to attain them and a few of the more dramatic results achieved. It should not only answer the questions of interested amateurs, but should excite the curiosity of those outside archaeology altogether. So many facts and original ideas are included, without a compression that would make reading difficult, that a summary of the contents inevitably fails to do justice to the book's true riches.

Chapter I sketches briefly but acutely the history of Prehistory in its social context. The story of Buckland and the first ceremonial burial of Palaeolithic Age ever uncovered, but not discovered, is retold to emphasize what blinkers were lifted from scientists' eyes by the *Origin of Species*. The next chapter answers the inevitable question, bane of every excavator, 'How do you know where to dig'? Casual discoveries, air-photography, the use of maps, place-names and literary clues and the romantic tale of Dragons' Bones are profitably discussed. Under the heading 'Preservation' the author describes, not the activities of H.M.O.W. and similar agencies of conservation, but the effect of climatic and geological conditions on monuments and relics. Illustrations of the survival of organic materials, aptly drawn, not only from familiar regions like Orkney and Egypt, but also from Greenland, the Altai and Arizona, bring home vividly to the reader how much is normally missing in the archaeological record and how the prehistorian must fill the gap by cautious use of scientific imagination. The account of 'Excavation' develops this theme in showing how data for the legitimate application of imagination can be gathered by meticulous excavation. General readers and students alike will appreciate the inclusion

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of Stonehenge as an example. A fellow prehistorian may be allowed to remark that Clark rather over-estimates the advantages enjoyed by Romanists and Orientalists in excavation, but omits their most decisive help to reconstruction—the existence of figured monuments like Trajan's Column and the temple models from Uruk-Warka.

The foregoing chapters appeal primarily to the laymen in clothing with life the dry bones of archaeological processes. But they are of real service to specialists and students in introducing them to unfamiliar discoveries like the mesolithic sledge-runners from Finland, the Pazurik tombs, and the Basket Makers, and in collecting and illustrating materials and observations, often referred to but actually accessible in only a few large libraries, like the Ladby Ship, the Fayum cereals and silos, Dutch palisaded barrows, a Skara Brae house. The succeeding chapters, on the contrary, while equally illuminating to the general reader, provide for the student a text-book of principles, stated with a precision conspicuously lacking in more pretentious manuals. The chapter on chronology gives precisely what a university student needs to know. It explains under relative chronology, typology (illustrating both evolutionary and degenerative series), stratigraphy, geological, palaeontological and botanical methods, distribution maps (emphasizing the contrasted conclusions to be drawn from comparing distributions of cultures on the one hand, of individual types on the other) and synchronism (the correlation of Western and Danubian sequences agrees nicely with that given in the new edition of my *Dawn of European Civilization*, just published). For determining absolute age geochronology (both Milanković's radiation curve and de Geer's varves), dendrochronology and historical connexions are considered. (It is a pity that Neugebauer's debunking of 'the earliest certain date in human history' was not mentioned in explaining the Sothic cycle—an account of it is given in the latest edition of *Man makes Himself*.)

Still of course a student can find all this information somehow if he hunt through sufficient text-books and journals. The principles of interpretation, enunciated and concretely illustrated in chapter vi, can be found nowhere else, unless perhaps in Russian, and rather distorted by undue reverence for the good Morgan. In a masterly section of 38 pages the author gives hints on how scientific archaeology may reconstruct the economic bases of prehistoric societies, relate them to material and spiritual equipment, estimate population densities (a crucial subject hitherto ignored), and thus lay foundations for legitimate speculations on social behaviour, aptly exemplified by a quite novel account of head-hunting and cannibalism in prehistoric Europe. The last chapter, bearing the same title as the book, is in fact mainly devoted to the abuse of archaeology by nationalist, communist, racialist and imperialist dogmatism. In my opinion

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the disabilities imposed by Nazism have been unduly minimized, those due to Soviet communism exaggerated by reliance on tainted sources; and the plight of archaeology in Great Britain is not due to the divorce of the people from the land by the industrial revolution, but to government by an oligarchy imbued with the ideology of feudal barons and oriental satraps. But the real point is well stated in the end. 'To see big things whole they must be seen from a distance, and that is precisely what archaeology enables one to do. The history of mankind, when any phase of it is studied at close quarters, appears to be a maze of apparent inconsequences: it is only when viewed from the perspective of prehistory that the broad sweeps become easily apparent and the history of men gives place to the history of man'.

The book is illustrated with 24 half-tones and 31 line drawings, diagrams and maps. These depict precisely the subjects an experienced teacher wants to show his students but often finds it hard to lay his hands on. Now the students will be able to possess the rare illustrations themselves and study them in the blacked-out nights. The publishers deserve our gratitude for producing this work so cheaply and so well-illustrated. They will doubtless be rewarded by large sales; for this is quite the sort of book that anyone will like to buy for reading in long dark evenings of isolation and alarm. V. GORDON CHILDE.

ANNUAL REPORTS OF THE ARCHAEOLOGICAL DEPARTMENT OF H.E.H. THE NIZAM'S DOMINIONS: 1934-5, *edited by* SYED YUSUF, Assistant Director, *pp.* ix, 92, 8 *plates*; 1935-6, *edited by* G. YAZDANI, Director, *pp.* ix, 79, 15 *plates*. *Calcutta, Baptist Mission Press, 1938.*

In the first of these reports the chief items of interest are Kalyani, Warangal and some inscriptions.

Kalyani in 1100-1200 was the capital of the Hindu empire of the Chalukyas of the West Deccan. Its earlier history is not known; the inscription cited on p. 17 belongs to the twelfth, not the sixth century. The Muslims, appreciating its strategic value on the Kanarese-Telugu border zone, elaborated its defences, which still stand intact. Fifteen inscriptions, dating from 1400-1700, prove their interest in the place. The only relics found of the Hindu period were 3 inscriptions (no details given) and some unusually attractive figure sculptures (pl. 2). The dates 1635 and 1657 on p. 18 are apparently misprints for 1657 and 1687.

Warangal was the capital of the Hindu Kakatiyas, rulers of the East Deccan, Telingana. Excavations in the fort, in the area between the four well-known *toranas* (gateways) of the temple precincts yielded some fine sculpture, but the ground plan of the temple to which they belong is 'incomplete'.

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The inscriptions, all from East Deccan, are edited by Dr P. Sreenivasachar, whose notes throw an interesting light on the struggle between the Deccani Sultans, and the Hindus of Orissa, Telingana and Vijayanagar for the control of the Kistna and Godavari Deltas.

The Report for 1935-6 contains notes on the fort of Mudgal, in the blood-stained Raichur Doab between the Sultanates and Vijayanagar ; on two other forts, in the Ajanta Range ; on a newly discovered cave with Hindu bas-reliefs at Bhokardon near the battlefield of Assaye ; on three groups of Chalukyan temples, Gabbur, Katchapur and Kondapaka ; and on a Kakatiya inscription at Raichur of 1294, the year of the first Muslim raid into the Deccan.

Excavations at Maski, the site of an Asokan inscription of 3rd cent. B.C., produced a tantalizing series of celts, chert flakes, potsherds, figurines, terracotta discs, beads and chank bangles (pl. 3-6), but no stratigraphic evidence of culture sequence. An exhaustive study of such finds all over India is first necessary if ever these objects are to be used for dating. Palaeolithic types are reported, but none are figured. A curious adaptation of the 'triskeles' motive is shown on pl. 7 from a temple near by, of three (not 'two') geese with their necks intertwined.

In spite of the devastations of road contractors, South India perhaps still holds more 'megalithic' monuments than the whole of the rest of the world. For several decades this subject was shelved ; interest was revived by the enterprise of Dr Hunt, the late Capt. Munn and Mr Yazdani. The vast dolmen fields of Rajankalur and Haggeritgi, described by Meadows Taylor in the sixties of last century, have almost entirely disappeared, but the Benkal group (pl. 8) which survives is truly 'of exciting interest'. The Department is busy listing sites and providing for their protection. In the absence of a map the lists are not easy to follow. The dating of this 'megalithic' culture is the subject of wild speculation ; '3000-1000 B.C.' convinces no one, and the attempt to date it by the Rig-Veda (p. 31) is pathetic. Fergusson thought the Shahpur cemetery could not be earlier than 14th cent. Mr Codrington, on ampler evidence, suggests from about 3rd cent. B.C. to well on in the Christian era. (*Man*, 1930, no. 139). In any case a careful study of all available evidence is a condition precedent to tackling this problem. The 'pottery marks' of this culture, by the way, were published by Bruce Foote in 1901, in his Madras Museum Catalogue.

Monographs are in preparation for Bidar and Gulbarga, and a much-needed list of 'unpublished' inscriptions, which it is to be hoped will include full references to those that have been published.

The reports are well printed, the plates excellent. Mr Yazdani is to be congratulated on the progress made.

F. J. RICHARDS.

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ARCHÄOLOGISCHE SIEDLUNGSKUNDE DES TRIERER LANDES.

By JOSEPH STEINHAUSEN. pp. xvi, 614, with 46 plates, one coloured map and 25 maps in text. Trier : Paulinus-Druckerei, 1936. 9 marks.

The justification of this book is the importance of the area with which it deals. While showing a rich and interesting agricultural life of its own, it acts as a transmission zone of eastern and western borne cultures. It is, on the one hand, among the earliest recipients of northern Christianity ; on the other, the western outpost of Germanic speech, facts which reflect its cultural behaviour throughout the ages.

Dr Steinhausen's work describes this cultural behaviour from the earliest times to the end of the Merovingian period. It is mainly compilation, excellent and planned on a scale so large that one's principal regret is for a defect in completeness ; the author seems to have fallen between the stools of commenting merely upon that portion of the Rhineland archaeological map already published (Trier-Mettendorf) and of offering a complete survey of the Trier region. When the geological record is thus 'sampled', the reader feels its inadequacy on trying to relate distribution-maps (of which Steinhausen publishes many) to the general structure.

On the whole, the work has all the merits and some of the defects of a compilation. Steinhausen's net is so wide that it is not easy to discover what his real *expertise* is ; nevertheless it may be suspected that he is not a prehistorian. It is true that Roman remains have rather overshadowed prehistoric finds in this region, so that few settlement-sites are known and even recorded burials are not very common until the Iron Age. Yet the author's insistence on 'prudence in everything' and his reluctance to draw conclusions from such finds as there are seem to reveal a lack of grip upon the comparative material. Like the diplomatist, the prehistorian needs the 'tacte des choses possibles'. We discern, however, the clash of eastern and western cultures in a neolithic which shows Camp de Chassey affinities in its tools, and Peterborough pottery from the one camp as yet studied (Dietzenley) ; urnfields and Hallstatt tumuli illustrate movements from the east at later dates ; and the author subscribes with cautious reluctance to the Hawkes-Dunning thesis of a German infiltration represented by cremation interments.

With the Roman period, the remains become numerous enough to permit conclusions on their own merits, and Steinhausen's work gains in strength. The origin of Trier itself presents a difficulty and the author has only had time to insert in a footnote the new views of Koethe ; its great industrial period dates from c. 70-250. It is important in itself for its great ceramic manufactures, and serves besides as a distributing centre by way of the Moselle river and the Roman roads for the consumption needs of the military occupation.

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The countryside well illustrates the sudden rise of prosperity which Dunning and Hawkes observed in Belgica as a whole. Settlements now appear on the originally wooded Keuper and the Devonian of Hunsruck and Eifel. It is the epoch of the Secundinii at Igel and the Neumagen tombstones (which are no longer regarded as having been carried from Trier). Rostovtzeff's supposition of foreign capitalists responsible for this development is unnecessary and inconsistent with the epigraphic record. Foreigners are rare among the Treveri; they are the Scots of Roman Gaul.

This prosperity comes to a violent end, far more catastrophic than one had imagined, with the barbarian invasions of the third century. The restoration of Diocletian and Constantine was a titanic work here, indeed much of the ground lost was never recovered. Nevertheless in the fertile Bitburg neighbourhood (where Steinhausen sees an imperial domain in the *Landmauern*) reconstruction was successful, and there is even occupation *de novo*. With the establishment of the system of defence in depth, Trier itself gains a new importance. It is now a key point in the road system upon which this defence was based and becomes furthermore the seat of the praetorian praefect and frequently of the emperor.

The Merovingian period poses the problem of continuity, and though this is a far better documented region than the world of Gildas, Nennius, and the Chronicle, the problem is no easier. In Trier itself the bishopric survived, and it is believed that the Franks occupied an area distinct from that of the Gallo-Romans within the wall (an interesting parallel to what Wheeler has supposed for London); in the country Frankish remains are only very occasionally found in Roman villas, and continuity was certainly merely the affair of the lowest classes, as is well shown by the vocabulary transmitted. Glass-working and pottery preserve Roman technical terms and even Roman technique (Steinhausen might perhaps have made more of this); but smithing, weaving and even agriculture are linguistically German henceforward. Moreover, the picture of settlement shows a return to prehistoric conditions, and parallels neatly enough Fox's Cambridgeshire maps. The extent of continuity is well illustrated by the *-acum* place names. Not a few are actually preserved among the *-ingens* and *-heims* of the Triassic lands, but the suffix never becomes, as in western Gaul, a live unit in the formation of new estate names.

There are many more interesting points which are suggested by this very full study. It is not perhaps a book for the general reader; its value, however, to students of other regions as a standard of comparison is immense. And though one might press for more plans and some pictures of objects instead of photographs of unimportant villages (there is not even a proper town-plan of

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Trier), the references are all there—in thousands. Dr Steinhausen may be congratulated on a fine monument of industry, and his publishers on the very moderate price at which they have issued it.

C. E. STEVENS.

1. MOUNDS IN THE PLAIN OF ANTIOCH: an archaeological survey. By ROBERT J. BRAIDWOOD. *University of Chicago, Oriental Institute Publications*, volume XLVIII.
2. ANTIOCH ON THE ORONTES II: The Excavations 1933–1936. *Princeton University Press and Oxford University Press*, 1938. £5 15s.

1. In many parts of the East the surface is blistered with ancient mounds. In the low-lying ground around Antioch the author of this book has plotted 178 mounds in an area of 535 square kilometres. They are of various sizes: a large one measures 400 m. long and rises perhaps 30 m. high; a small one is only 25 m. in diameter. Many are the sites of old towns or villages, some of isolated buildings, others may be mere dumps. The book is an inventory of the potsherds collected from these mounds in a reconnaissance which took three weeks. Care was taken to include samples of every type, plain as well as decorated, and the collections were studied mainly in the light of a series of stratigraphic records which had been obtained by the excavators of one of the mounds in question, Tell al Judaïdah, where fourteen cultural periods had been distinguished. To complete the historical framework, two recent groups, one or two sub-groups of foreign wares, and a much too long Medieval-Arab group, stretching from A.D. 600 to 1800 had to be added. The result is presented as an archaeological survey of the area.

What is the value of such work? The author, Mr Braidwood, is under no illusions as to the limited validity of any conclusions which can be drawn from his data. The data prove that the plain was very heavily occupied during the Hellenistic-Roman periods and the Medieval-Arab period: the pre-Hellenistic periods for which material is most abundant are—Judaïdah XIII (the beginning of the appearance of metal), XI (3100–2600), X (2600–2400), VII (1800–1600), V (1200–1000), IV (1000–500). But no one would be justified in concluding that all the intermediate periods were times of relative poverty, the Late Bronze period (1600–1200), for example, or the Early Christian periods (A.D. 350–600). Arguments *ex silentio* are always dangerous, but peculiarly so with material of this type. But the work was none the less worth doing, and it has been very well executed. The 27 maps and 9 text figures are beautifully reproduced and the letterpress is brief and sensible. The writer points out that there are several mounds in areas now swampy but none in what is now a lake, and it is suggested that the water table has risen, what is now a swamp was formerly well drained, and what is now a lake was once in all probability a swamp.

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2. The second book deals with excavations at Antioch itself. A joint American-French expedition has been working here since 1932 : in Antioch and the neighbourhood they have dug on 40 sites, and in Daphne and its surroundings on another 24. Work in the port of Seleucia is also contemplated. The present volume is the second which has been published, but the amount of information which has been released up to date is still rather small. This may be disappointing, but it is probably wise. The book before us contains detailed accounts of a Roman theatre at Daphne by Donald N. Wilber ; of the water system leading from Daphne to Antioch, also by Wilber ; of an important early church by Jean Lassus ; of a large villa at Daphne, also by Lassus ; the Greek and Latin inscriptions by Glanville Downie ; the Arabic inscriptions by N. A. Faris. Besides these, which may be regarded as more or less final publications, there are illustrated catalogues of the sculptures and figured mosaics found up to date, without, however, any critical discussion. We are glad to learn from the foreword by W. A. Campbell, the Field Director, that the island, the main street and the cemeteries have been located, and we look forward to reading about them in some future volume.

The theatre at Daphne was built apparently towards the end of the first century A.D. Arrangements existed to flood the orchestra for water displays ; a few fragments of fine architectural detail give promise of the quality of workmanship which may be expected elsewhere in the city ; very little of the building was left, and that little found under considerable difficulties, trees which had to be spared and so forth. It has now been wisely buried again.

The church was found on the right bank of the Orontes outside the limits of the city. There were no trees to obstruct work and the whole monument was cleared. Unfortunately it had been stripped everywhere to floor level or lower. The building was cruciform, a central square measuring 16 m. by 16, with four equal arms, 11 m. across and 25 m. long, pointing to the cardinal points. In the central part there were traces of a tribune in the middle and two graves in the corners on the north side. In the four arms the mosaic floors were fairly well preserved near the centre of the monument but broken towards the extremities, in part by later burials. The patterns of the mosaics are all geometrical in character : those in the eastern arm are very like some of those found by Harvey in the Constantinian nave at Bethlehem. Inscriptions were found in the north, south and west arms : on these, the arm in question is called in each case an *exedra*, and the floor in the north *exedra* is dated A.D. 387—a date close to the date of Bethlehem. In the east arm no inscription was found and we do not know whether the term *exedra* was applied to this arm also.

The plan is extremely interesting and M. Lassus is almost certainly right in regarding it as the martyrium in which the remains of St. Babylas ultimately

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found rest. A learned note on the translation of the body of St. Babylas, mentioned by Chrysostom, Sozomen and others, is contributed by Mr Downie. The dates fit, and the site is appropriate ; one of the graves in the centre may be identified therefore as that of St. Babylas, the other as that of Bishop Meletius, the builder of the church.

The tribune in the middle is reconstructed by M. Lassus on the analogy of a tribune at Rusafa, with an apsidal synthronus at the west and an altar to the east, for use, one would suppose, on the festival of the saint. This is quite possible, though the material evidence is slight, but I hesitate to follow M. Lassus further in regarding the eastern arm of the church as an arm in all respects like the others—the three *exedrae*, as we may call them. It seems to me more likely that there was an altar and sanctuary in the eastern arm, whether there was also one in the middle or not. There is, admittedly, no trace of its existence but there is equally nothing to negative its existence, and the west-east axis of the whole building in the form which it ultimately assumed, when various annexes were added—a porch at the west end and other buildings, among them a baptistery, on either side of the east arm—is so strongly accented, as the writer admits (p. 22), that it would surely be more reasonable to assume that this arm ended, after the normal fashion, in an internal apse.

Whatever doubts there may be on these points, there can be no doubt about the importance of the find and the care with which it has been studied.

The second monument which M. Lassus describes with equal minuteness, is the great villa at Yakto in the Daphne area, where the mosaic with the topographical border was found (see *Antioch*, 1). The original building goes back to the third century but was rebuilt and enlarged just after the middle of the fifth century, and little more than the plan of a section of the first house and one very fine mosaic, with a representation of birds, has survived. The second plan is a rambling affair, consisting of four more or less separate buildings loosely connected together, the country house, according to the editor, of a wealthy oriental, planned to suit oriental conventions. There is, of course, the inevitable bath, but there are also two interesting cruciform rooms roofed, in part at least, with cupolas, and a third with two, if not three, apses. The clearance of this building, like the theatre, was obstructed by trees which could not be destroyed, and where there were no impediments, the walls were found to have suffered greatly. Nothing was standing more than a metre high or thereabouts and in places the line had disappeared completely and the plan was lost. Gold and coloured glass tesserae were found in the débris in some of the rooms which had probably been roofed with cupolas, and among the fragments was one which came from a figure-subject ; it had part of a face. Again, however, the chief finds are the floor mosaics and the pavements in

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opus sectile which were laid over the mosaics in a final transmogrification of the villa at the beginning of the sixth century. Only two more floors with figure-subjects are published in this volume: one represents a goddess on horseback, riding lance in hand to attack a lion, while a mounted attendant holds over her head a yellow parasol with a red fringe. The other, which is less gauche, represents Thalassa surrounded by boys fishing and cupids riding dolphins in the waves. Most of the floors are purely geometrical and about these M. Lassus makes two suggestive observations. In the first place, he suggests that there often seems to be a definite correspondence between the design of the floor and the type of roof—a circular pattern, for instance, may indicate a cupola. Secondly, he draws attention, both here and in the chapter on the church, to the way the colours are handled in regular gradations, either repeated or counter-changed, on a fairly rigid schème—white, light blue, dark blue, black, white, rose, red, black, coming over and over again in the same order.

The volume contains eighty large collotype plates which are beautifully printed—that is why it is so expensive. Twenty-two of the plates are devoted to sculptural remains, the rest to the figure-mosaics which have been found in the numerous sites that have been dug.

J. W. CROWFOOT.

THE ROUTE OF THE EXODUS. By A. LUCAS. *Edward Arnold. pp. 99 and map on end paper. 3s 6d.*

A new recruit is welcomed to the ranks of those who have attempted to throw light on the mystery of the Israelitish wanderings, and this is Mr Lucas, who has served in Egypt for the same period as Moses spent in Sinai. The author has taken a considerable amount of trouble over this small book, and has apparently studied at length every conceivable volume and document on the subject. He devotes the greater part of the first few chapters to locating the area in which the Israelites were dwelling previous to their flight, and, after weighing the evidence, holds the view that Kantir near the village of Fakis is the most likely spot.

Among other documents he examines is the translation of a Nineteenth Dynasty papyrus, which is the monthly report of a minor official stationed in the Wadi Tumilat. In this the official states 'we have finished causing the Bedawi tribes of Edom to pass the fortress of Merenptah towards the pools of Pithom . . . in order to feed themselves and their flocks'. This is particularly interesting because it proves how little things have changed in 3200 years. Today the Beduin of Edom still come in to Sinai and the Tumilat region west of Ismailieh to feed themselves and their flocks, and worried officials still endeavour to check their movements by compelling them to call at stipulated frontier-posts on the way. The Beduin of the Nineteenth Dynasty apparently

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objected as strongly to government interference as he does in the twentieth century.

The route of the Israelites when they left Egypt was, Mr Lucas maintains, the centre track across Sinai known as the Darb el Haj or the Pilgrim's Way, and in selecting this he examines the evidence in favour of the alternatives, *i.e.*, the accepted route to the south or the northern track along the way of the Philistines direct towards Palestine. He finds nothing in favour of the reviewer's particular theory that they passed along the strip of sand that divides the north of the Bardawil Lake from the Mediterranean Sea. This, he says, is impossible as it is waterless the whole way, lacking in pasturage and liable to inundations. Mr Lucas cannot have travelled by this route himself to spy out the land otherwise he would remember the halfway halt now called Gals, which in Roman days was known as Mt. Casius and was of some importance. Here two waterholes exist and there is a large area of grazing up to the standard of that to be found in any other part of Sinai. Other grazing areas exist at Gatafa and Likleifa, and the track is not subject to general inundations, but only to cuts made by the sea at the narrowest spots. These cuts do not prevent the passage of travelling Beduin today and it is only in the wildest weather that they are flowing.

It is disappointing to find that Mr Lucas has no particular theory to explain the loss of the Egyptian host, and he side-tracks the subject by accepting tentatively the view held by Scarth and others that a high wind displaced the waters in one of the bitter lakes in the vicinity of the Isthmus of Suez, driving them back sufficiently to provide a dry crossing for the Israelites and returning to swallow up the Egyptians. Having shot snipe and duck over most of these big brackish lakes of Egypt during twenty years the reviewer has had some experience of this heaping up of the water owing to the action of violent gales. There is undoubtedly a change of levels sufficient to cover the promising snipe marsh of yesterday with four inches of water today. It must be understood, however, that this pushing back of the water, though it is sufficient to interfere with the feeding habits of a small bird like a snipe, is hardly of such a nature as to account for the loss of a whole army of husky Egyptian soldiers.

Mr Lucas takes the host by the central route across Sinai direct to Akaba, which he locates as the *Yam Suph* (sea of Reeds) though he admits there is little sea-weed there. Having traced them to Akaba he is faced by the quail problem, because it is the alighting of these birds on the Mediterranean coast that causes many people to accept the northern route in preference to the south. The author, however, maintains that Akaba is on the line of flight of the migrating quails, and holds the view that a violent wind must have forced them down for the benefit of the Israelites. This may or may not have happened, but the fact remains that the quail today do land on the Mediterranean coast in hundreds

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of thousands every year, and that only a few tired or sick birds are seen south of this area. Mr Lucas states that as the area is uninhabited they may come down now and escape notice, and with regard to this he should make the acquaintance of those keen financiers, the Akabawi and the local Howeitat. If there is any profit lurking in their neighbourhood it would be a miracle if it escaped their notice.

A considerable portion of the book is devoted to the question of manna, and Mr Lucas accepts the theory that it is the exudation from the local tamarisk or *tarfa*. As an analytical chemist he has examined present day manna and finds that it is mostly sugar, that it cannot be ground in a hand-mill, but it can be seethed in a pot.

The Holy Mount he identifies as Gebel Baghir in the Wadi Ithm above Akaba, and if one accepts this area as the site of the wanderings this mountain will pass the test.

The book is a well-reasoned argument based on close study of documentary evidence and is well worth consideration. The fact remains, however, that the wanderings—if they were actually wanderings—must still remain a mystery, as no one yet has been able to accept that view and explain how the Israelites found fodder not only for their sheep and goats, but also for oxen, and, what is also to the point, routes on which ox-waggons would travel. C. S. JARVIS.

CAHERCOMMAUN: a Stone Fort in County Clare. By H. O'NEILL HENCKEN. (Extra volume of the Royal Society of Antiquaries of Ireland). Dublin: John Falconer, 1938. pp. 82, with 11 plates. 5s.

This is the record, and an admirable one, of the first thorough and scientific excavation of an Irish caher or stone fort ever carried out. So long ago as 1825 the remarkable Staigue Fort in Kerry was cleared, and the structure itself described in detail and with fair accuracy, but the interior does not seem to have been excavated nor was any conclusion as to the date of the fort arrived at. Between 1870 and 1875 Grianan Ailigh, the similar structure which crowns a hilltop in Donegal not far from Derry city, was examined (and partially rebuilt) but neither of these operations can be described as complete or scientific investigations, and the dating of the structures remained uncertain. It is obvious that the excavation of sites so extensive as the larger forts called for large resources and these, till the coming of the Harvard Archaeological Expedition to Ireland, were not available in that country. The success of the operations of the Expedition and the attention which it drew to the archaeological wealth of Ireland was, in great measure, responsible for the granting of State aid to archaeological investigation there, and for the inauguration of an extensive programme of excavation which has been in progress since 1934, the year of the excavation of Cahercommaun.

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The fort itself is in the Burren of Clare, an extensive limestone plateau occupying the northern part of the county and covered with structures of the kind, large and small, to the number of three or more to the square mile. From the tabular exposed outcrops of well-stratified rock an ample supply of stone is readily obtainable, and of a nature admirably suited to the construction of stable, though unmortared, walls of stone. This was doubtless the prime factor in the development of the local type of building. While the majority of the Burren forts were only steadings Cahercommaun was a fortress also, but it does not seem to have been occupied for more than a few generations. It has three rings, the outer walls abutting on the edge of the ravine and enclosing areas, radially divided, which seem to have been used for cattle; the innermost wall is very massive and roughly circular in plan. There are two souterrains (one with a foundation deposit) within the fort and the traces of a dozen irregular and poorly constructed buildings, besides numerous hearths which, with the large quantity of animal bones found, mainly of oxen, afford a picture of a life of rude plenty. Bronze finds were rare but iron and bone plentiful. The fact that it is datable to the ninth century and is thus contemporary with some of the crannógs—such as Lagore, also excavated by the Expedition—tempts to the conclusion that similar structures—the great forts of the Aran Islands, Staigue, and the Grianan, for example—must also belong to the period. The discoveries, however, of Late Bronze earthworks in Limerick (Cush), a crannóg of the same age in Clare (Knocknalappa), and a fort datable to about 350 B.C. near Turoe in Galway, call for caution in generalizations as to fort datings. The ring-fort, whether of earth or stone, probably has a quite respectable heredity as well as, like the crannóg, a long history of later use.

The volume is amply and clearly illustrated by the photographs and plans, and sections to large scales which accompany the description and analysis of the excavation, which are both lucid and are from the able pen of Dr Hencken himself. He and the Society are to be congratulated upon the excellence of the special volume. Dr H. L. Movius contributes a short section on the human remains, while Mr Stelfox and Miss G. Roche deal with the animal bones.

H. G. LEASK.

PREHISTORIC POTTERY IN CHINA. By G. D. Wu. *Kegan Paul & Co.*, on behalf of the Courtauld Institute of Art, University of London, 1938. *pp.* 180, 67 *plates*, 2 *maps*, and 5 *tables*. 21s.

The author has had wide experience of his subject, in both field and study. Hence his book provides a welcome addition to our knowledge of early Chinese ceramics, and the views which it expresses will receive thoughtful consideration from archaeologists everywhere.

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Mr Wu (p. 171) states it as his purpose 'to make a systematic study of prehistoric pottery in China in order to formulate a chronological sequence of prehistoric sites'. To this end he groups the objects of his investigation under three successive heads, *viz.*, red, black, and grey wares. Of these, the first two are entirely prehistoric; while the last, according to Mr Wu, belongs especially, though not exclusively, to the dawn of China's history—the period of the Shang Dynasty (roughly the latter half of the second millennium B.C.)

The author has taken great pains in the organization of his material, and his presentation is detailed, yet clear. The greater part of his text consists of a minute and accurate description of the appearance, characteristics, and probable methods of manufacture of the three major types of early Chinese pottery named above. Mr Wu's close observation and acute analysis of these deserve high commendation. There is also a brief but well selected bibliography (consisting chiefly of Chinese titles), while a carefully prepared index greatly facilitates reference. The two maps are helpful and truly illustrative, and the excellent plates, grouped at the end of the book, form an admirable complement to the text.

Mr Wu has performed the first part of his stated purpose—a systematic study of early Chinese pottery—exceedingly well. With the second part—the establishment of a chronological sequence of prehistoric sites—he has not, in my opinion, been equally successful. To base such an attempt, indeed, on a study of a single class of material remains, to the avowed exclusion (p. 3 ff.) of all others, is an extremely hazardous proceeding, unlikely if not impossible of accomplishment.

Perusal of Mr Wu's book will probably leave most of its readers with the impression that his two earlier (prehistoric) periods were characterized, respectively, by the red and the black pottery groups alone. That such was however far from the case, my own observations in the field have led me to conclude. My experience, in direct contradiction to Mr Wu's statement (*cf.* p. 158), has been that it is the grey pottery (Mr Wu's class 3) that occurs in greatest abundance on all ancient Chinese sites, without exception, until shortly after the beginning of the Christian era. On the oldest of these it appears alone; while later it is mingled, in varying but always greatly predominant proportion, with the other types.

Hence the rather sophisticated 'red' pottery, made (in part at least) on the 'turn-table' or *tournette*, can hardly have been the earliest known in China. Mr Wu, incidentally, decides (p. 22), on what seem insufficient grounds, to drop the old name of Yang Shao (from the type-site), bestowed on it by its discoverer, Dr J. G. Andersson. To replace established nomenclatures,

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however, save for reasons of the utmost cogency, is certain to cause confusion, and should be avoided wherever possible.

In reality the oldest ware yet found in China appears, from the evidence afforded by actual excavation, to have been a coarse, thick variety, entirely shaped by hand (most often by the 'coiling' process), and pretty surely ancestral to the author's grey pottery. It belonged in a broad sense, most archaeologists seem agreed, to the same ceramic family that existed in so many other parts of eastern Asia during Neolithic times. It was invariably ornamented, if at all, by modifying its surface in various ways with incised or impressed markings or by luting on strips or lumps of moist clay before firing.

This primitive-looking grey ware has an exceedingly wide distribution throughout northern China, where it occurs, moreover, at all depths. The sites on which it appears alone, to the exclusion of other types of pottery, often lie off the main thoroughfares of travel, up lateral valleys or on the higher and therefore older river-terraces. Further, in many regions it is the ware found associated with interments—a fact in itself indicative of considerable antiquity, on account of the workings of religious conservatism.

This grey pottery, moreover, so far from having been absent from Mongolia until the historical period (*cf.* p. 156), seems, to judge from the observations of non-Chinese investigators, to go back to an ancient sedentary planting population with a culture not dissimilar to that of the Neolithic northern Chinese themselves. Everything points to its having far antedated the historical period, although, just as in China proper, it doubtless survived into it.

Mr Wu's 'red' type, on the other hand, is never, in my experience, found alone, but always in conjunction (though in relatively far smaller amount) with the coarse grey pottery just described. Its distribution, however, is far more restricted than that of the latter, and seems to have some sort of connexion with the main routes of migration and travel; for it is invariably on or near these that it occurs. It seems however to have had associated with it no distinct culture-traits of its own, save its decoration by burnishing and in some cases by the application of painted patterns; and by the use, in its manufacture, of the *tournette* or 'slow-wheel'.

Mr Wu dismisses with a few words (p. 15 ff.) the opinion of many competent Occidental archaeologists that this 'red' ware—sometimes called the 'Chinese Painted Pottery'—bears a genetic relationship to similar wares found at the opposite end of the famous 'corridor of the steppes', in western Asia and eastern Europe. The point would seem to have called for more detailed consideration on Mr Wu's part.

The author's discussion of the wares included in his second prehistoric period—that of the black pottery—is very good; though here too he should

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have made it clear that this type never occurs alone but always mingled with the old coarse grey variety. He might also have told us more than he does (*e.g.*, on p. 11, quoting Dr Li Chi) in regard to the associated finds—for example, town walls of *terre pisé*, the true potter's wheel, and hitherto unknown domestic animals; since most of these culture-traits are already long known in the ancient Near East but new in China.

There appears, incidentally, a contradiction in the text here. Mr Wu says (p. 135) that while the wheel was used in the black pottery period (a conclusion to which I have long adhered; *cf.* my paper, 'The Neolithic Age in Northern China', *ANTIQUITY*, 1933, VII, 400), it was later replaced by other methods of manufacture. Yet in the immediately following paragraph (same page) he states that the superiority of the wheel was so overwhelming that when it appeared it soon drove out the older techniques of pottery-making. The truth seems to be that much of the old Neolithic culture survived, among the Chinese peasantry at least, until after the commencement of the Christian era; and that both more and less advanced techniques of potting thus persisted side by side—as indeed they still do, at the present day.

In speaking of the potter's wheel in China, to state any definite conclusions as to its absence in the northwestern Chinese province of Kansu, as Mr Wu does (p. 135 again), seems unsafe in view of our extremely limited knowledge of the archaeology of that region.

Mr Wu well describes (p. 41 ff.) the very fine Shang Dynasty 'white ware', much of it engraved with the same motifs that appear on the contemporary bronze ritual vessels. He includes this type, by inference at least, in his grey pottery period. Many authorities suspect that the 'Shang white ware' pertained more particularly to the ruling class of the day. In point of time, at all events, it was associated with a whole group of new culture-traits, including the growing of wheat, the casting of bronze (though not yet of bronze swords), and the use of the horse-drawn chariot, all of which appeared in China about or perhaps shortly after the beginning of the second millennium B.C.

There is an oversight in the mention (p. 6 and *cf.* also p. 71) of Dr Li Chi's and Professor P. L. Yüan's expedition to Shansi province in 1926. Mr Wu has here omitted to state that it was undertaken at the behest of the Freer Gallery of Art of the Smithsonian Institution, by which organization Dr Li was then employed.

The foregoing paragraphs are in no sense intended to detract from the really great value of the book. As an accurate and objective description of the various forms of pottery found on early Chinese sites, it is, in my opinion, among the best that have thus far appeared.

C. W. BISHOP.

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THE ARCHAEOLOGY OF CRETE: an introduction. By J. D. S. PENDLEBURY. pp. xxxii, 400, with 43 plates, 24 maps, Methuen, 1938. 30s.

This book deserves the highest praise. It has long been badly wanted and it will remain a standard work for many years. Mr Pendlebury has set methodically to work to give archaeologists all the information that it is possible to collect about the island and its history from Neolithic to Roman times. His method is to deal with each archaeological period in turn, illustrating it with a distribution-map and a gazetteer-list of sites and finds of that period. That is a very great convenience for the research worker. It is a matter of moments only to lay his hand on the information he is seeking. No praise can be too high for such a careful and exceedingly painstaking list of sites. Future excavators will find them invaluable.

The author sketches the main characteristics of each period. The Neolithic age begins in an already advanced state of development. It was not indigenous, but shows strong Anatolian affinities, and some Egyptian connexions. No contact with the nearer Greek mainland is perceptible. The fantastically early date often given to the first arrival of the Neolithic settlers is here brought down to 'a few centuries before 4000 B.C.' at the earliest. Anatolian influence is more marked in the Early Minoan period, and settlements are near the sea. In Neolithic times life was precarious and dangerous, and settlements were in defended regions or well inland, in caves and rock-shelters. Mr Pendlebury views with sound scepticism the attempts to date E.M.I. from the occurrence of Egyptian hard stone vases which, as he says, are 'used for hundreds of years after their manufacture'. He also emphasizes the racial continuity of Cretans, and notes that 'from an anthropological point of view there is no evidence from E.M.I. to L.M.III that any change of racial type took place'.

In M.M.I come the first elements of the great palaces and the beginning of Minoan civilization properly so called. We are shown in detail the advances of all the arts and crafts, of architecture and of culture as a whole. Admirable design-charts show the most popular decorative motifs of each age.

With L.M.II the story of the greatness of the Minoan world comes to an end. The catastrophe is complete, and Crete continues its life as an appanage of the Mycenaean world, finally sinking once more into barbarism, with petty chiefs living in rock castles defending themselves as they can. Writing and the arts have long perished. The palaces remain in ruins unvisited because of some *tabu* that surrounded them. Crete is 'almost completely cut off from intercourse with the world outside the Aegean' and life is almost identical with what it was some 3000 years before in the Neolithic age.

Such is the cycle of Cretan history, a complete unity in itself, admirable as a yard-measure for our study of Aegean and Levantine life as a whole. Mr

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Pendlebury has carried out a most difficult and complicated task with the maximum of skill, and he has maintained a readable and lively style of writing throughout, so that the reader's interest never flags.

Naturally there are innumerable points that raise discussion in detail. The following occur to me as worth consideration. Mr Pendlebury adheres to the usual view that the Phaestos disk has an Anatolian origin. But his illustration (fig. 19, 1), shows an M.M.I seal with a female figure identical with those of the disk. And he omits reference to the Arkhalokhori inscribed axe which has an inscription very close to the Phaestos signary. There is also the Mallia stone inscription (Marinatos, *Arch. Anz.* 1937, p. 230) also omitted, which provides further evidence for a special hieratic script of which the Phaestos disk is probably an instance.

The Mallia broadsword (p. 164 and 272) is noted as an unusual type of weapon. It certainly has no parallel. Hall's explanation of it as a proto-Shardana weapon should have been mentioned, for Mr Pendlebury has no alternative to offer. The Marseilles oenochoe (p. 223) can hardly be described as the 'most beautiful vase ever made in Crete'. Its spiritual and artistic home is the Great Exhibition of 1851. The inscription on the silver bowl from Shamra (p. 225), here called Minoan, is called Cypriot by Schaeffer.

The author does not think that earthquakes can cause fires in the cities they destroy (p. 228). But that is precisely what they do, by overturning lamps or ovens. The Dreros bronzes are dated too early by some fifty years. The best analogy is the Karlsruhe bronze.

The old Black Sea Trade mirage is produced again (p. 231). Troy kept no such trade to itself, but got rich on the tribute and transit fees of the peoples who moved across the Dardanelles from Europe to Asia and vice versa. There is not a tittle of proof that any Black Sea trade existed at all in the Bronze Age. The Argonauts alone suggest it: but they apparently came back no richer!

Mr Pendlebury's description of the dramatic fall of Cnossos and the evidence for the last king carrying out his last rites (p. 231) suggests comparison with the last mass at S. Sophia before that city fell to the Turks.

The most likely explanation of the mention of Dorians in Crete in the famous passage in Homer is that the main invasion was probably preceded by various preliminary reconnaissances during the hundred years before the main attack (p. 260).

Mr Pendlebury takes a firm line of his own. He is so well versed in Cretan archaeology and history and has so wide a knowledge of the island, that his own views deserve the greatest attention. For instance, he rejects the Minoan connexions of the Atchana pottery (p. 174). He believes in the Asiatic and not the Nilotic origins of Minoan culture. Both the original stock and the later

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waves were all Anatolian (p. 279). He rejects the 'Blue Boy' (p. 134) and proves his simian nature beyond doubt. His views on the overwhelming influence of Crete over the mainland are quite clear. 'So Minoised does the rest of the Aegean become that it is impossible for the present writer to avoid the conclusion that it was dominated politically by Crete' (p. 225). He believes in a centralized empire that definitely ruled the mainland until the mainlanders turned on their masters and destroyed them. The fall of Cnossos and the other Cretan cities in L.M.II he attributes to a deliberate expedition from the mainland which came, destroyed and departed. His evidence for this view is convincing. He notes the curious lack of historical sense of the Cretan artist who never once records an event of note.

In short a very notable contribution to Minoan studies. And a particular word of praise for the series of photographs of typical sites of the different periods. The other plates are good and clear. Above all the maps are of great use. Published separately they would give a condensed history of Crete. Mr Pendlebury is to be warmly congratulated on an admirable piece of work.

S. CASSON.

CEMETERIES OF ARMANT I. By SIR ROBERT MOND and OLIVER H. MYERS. *Egypt Exploration Society, London, 1937. 2 v., pp. XIII, 300 and 78 plates. 25s.*

This work is a long, elaborate, and incredibly detailed publication of several seasons' work conducted by the Egypt Exploration Society's expedition in the desert to the northwest of the town of Armant in Upper Egypt, and deals mainly with the prehistoric remains encountered during those seasons. Three cemeteries, a number of scattered graves, and a settlement, all of the well known 'Predynastic' type and all badly plundered, were excavated with great thoroughness, as were also two brick tombs of proto-dynastic date. Scattered over the desert were found several handfuls of potsherds and agate artifacts, which have been attributed to an undatable, but presumably very early, 'Saharan' culture—a culture which is apparently traceable across north Africa as far west as Timbuctoo. In addition, there were discovered several sherds of Tasian and Badarian type ('Early Predynastic II and III'), some Old Kingdom and First Intermediate Period beads, two plundered tombs of the Middle Kingdom, and a quantity of fragmentary objects of the New Kingdom and Graeco-Roman Period, the latter including some Greek ostraka and some scraps of Coptic manuscripts.

The most interesting objects from the Predynastic graves are three plaques of painted gesso, which have been identified, with strong reservations, as either flags or model shields. There occurs to the reviewer the possibility that they

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might be fans of the stiff, revolving type represented in Middle Kingdom coffins and in the tombs at Beni Hassan.

The bulk of the material laboriously extracted from the remains of the cemeteries of Armant is pretty commonplace stuff from any point of view, and the valiant efforts of the authors of the publication fail to make it otherwise—the fully quoted opinions and analyses of the fifty specialists consulted and the host of registers, statistical tables, diagrams, charts, and graphs, which the book contains, all adding up in the end to little of real importance to either the layman or the student. As an exposition of the super-scientific method of recording and publishing archaeological finds *Cemeteries of Armant* is most impressive. From the point of view of results achieved it is not.

This is no fault of Mr Myers and his collaborators, whose enthusiasm, care, knowledge, and astounding energy merit the greatest respect and admiration. In the parts of the book where these gentlemen permit themselves to break away from the Armant finds proper and deal with broader phases of Egyptian pre-history, they have much to contribute to our knowledge and understanding of the period. This is particularly true of the sections written by Mr Myers himself, and of the excellent essay on the flint industry contributed by Suliman Huzayyin Effendi.

WILLIAM C. HAYES.

LANGUAGE HUNTING IN THE KARAKORUM. By E. O. LORIMER.
George Allen and Unwin, 1939. pp. 310, 24 plates and map. 12s 6d.

This book is an interesting record of day to day events. The specialist is advised by the author to consult the publications of her husband, Colonel Lorimer, for information regarding such topics as the origin of the people of Hunza and their language. The first five chapters are devoted to an account of the four years spent in Gilgit when Colonel Lorimer was the Political Agent there. It was during this period that the 'Language Hunting' began. Shina; Khowar, the language of the Chitral; and Burushaski, which is unrelated to any other language alive or dead, were mastered and recorded, while some material concerning Wakhi, the language spoken in the highlands of Hunza by immigrants from Afghanistan, was also secured. The ability to speak to a native in his own tongue is a great advantage and the author relates how, when her husband made a short speech in Burushaski at a Durbar 'there was no mistaking the amazement and delight with which the Burushaski speakers heard their language for the first time on the lips of a white man'.

The next two chapters deal with the time spent in England following upon Colonel Lorimer's retirement, when his three-volume work on the Burushaski language was prepared for publication, and arrangements made for another trip

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to Hunza for the further study of the people and their language. The Lorimers set sail in March 1934, ten years after the Colonel's retirement.

In part 2 of the book we are taken on the journey over the Burzil Pass to Gilgit and on to the beautiful valley of Hunza. The vivid descriptions of the author, aided by her excellent photographs, help one to realize the inaccessibility and isolation of Hunza, the most northerly state of India abutting on Afghanistan and Chinese Turkestan.

The remaining two-thirds of the book are given up to an account of the fifteen months spent with the Burusho, a primitive self-supporting people with neither machines nor money. The beauty of the Karakorum and the daily life of the people of Hunza are well described. The author was particularly qualified to deal with the latter for she could converse with the Burusho in their own language and possessed the power of gaining their confidence and love. We are introduced to the family life and are shown the people working at their various crafts and on their terraced fields, irrigated by water brought from a glacier snout. We are told of the crops they grow, how they make a threshing floor, employ cattle or flails for threshing, and winnow by throwing the grain in the air. Many difficulties, such as living at a height of 7000 ft. with very little level land; lack of rainfall, timber and manure; meagre crops, great summer heat, biting winter cold, and springtime starvation, are shown to be suffered manfully by these happy people so manifestly loved by the author. It is interesting to note here that nearly all the photographs of people show smiling faces.

J. W. PAGE.

EXCAVATIONS AT ASEIA IN ARCADIA 1936-1938. Preliminary report by ERIK J. HOLMBERG (*Göteborgs Högskolas Årsskrift* XLV, 1939). pp. 30 and 15 plates.

In a valley near the sources of the Eurotas and the Alpheios is a site at which Mr Holmberg made careful excavations which have materially added to our knowledge of neolithic Greece. He rightly observes that 'our knowledge of the Neolithic culture of southern Greece is so fragmentary that all contributions to our further understanding of it should be published as soon as possible'. And his publication is therefore the more welcome.

The excavations were carried out most scientifically, and the results are here admirably published. The author is to be congratulated on making a very considerable advance on our patchy knowledge of the period.

Aseia was inhabited from the Neolithic period down to the Middle Helladic. From then it remained desolate until late Classical times. The Neolithic pottery all comes from habitational strata and no cemetery was found. Remains of dwellings and hearths were not recognizable except in the Helladic strata.

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The pottery resembles but is not identical with that of the Neolithic strata of Corinth. Consequently Holmberg does not classify it according to the Corinthian classification of Weinberg. Actual Corinthian imports seem to appear in neolithic Asea, however, to make the distinction clear. A monochrome highly polished ware with a thick slip, and a burnished ware decorated with elaborate rectilinear patterns are the outstanding wares. The latter is, like so many Neolithic wares of Greece, of the highest technical competence. It is in close relation with the earliest Thessalian wares. At Asea there is no cultural break between the Neolithic and the Bronze Age. This is of the highest importance in view of the fact that elsewhere in the Peloponnese such a break is evident. The only cultural break at Asea is between the Early and the Middle Helladic. This is normal in the prehistory of Greece.

Asea will undoubtedly prove a type-site for Peloponnesian Neolithic. Small careful excavations such as these will solve the strange riddle of the origin of Neolithic culture in Greece.

S. CASSON.

HAITHABU, eine germanische Stadt der Fruhzeit. By HERBERT JANKUHN. Zweite erweiterte Auflage. Karl Wachholtz Verlag, Neumünster in Holstein, 1938. pp. XII, 207, with 207 illustrations and 4 plans.

Economic as well as political factors contributed to the importance which Haddeby—or Haithabu as Dr Jankuhn prefers to call it—enjoyed for almost three centuries. Not only did it lie, like its more northerly counterpart Birka, at the junction of two important routes, but also for long periods of its history it occupied the unenviable position of a frontier town between two strongly opposed political worlds. These circumstances combined to produce first the merchant town and second the complicated system of frontier works which cross the narrowest part of the peninsula between the Schlei and the Trene. This new and enlarged edition of *Haithabu* supplies an interim survey of the historical and archaeological evidence, designed as much to show the results of the excavations which have been carried out intermittently since the 1880's, and with more regularity since 1930, as to define the many problems which further excavation may be expected to solve. The author has not confined himself immediately to Haithabu but has included much material of a more general kind. The work is well supplied with plans and illustrations, but there are one or two omissions. None of the several plans of the town show the sites of each season's excavations. The exact position of the cemeteries as well as of the houses uncovered in the different parts of the town can only be guessed, nor is there any means of determining how much of the town yet remains to be excavated. Again, the carefully drawn section shown in fig. 50 would have been made much more intelligible by the inclusion of an explanatory legend.

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The occurrence of sherds of Rhineland *rollstempelkeramik* at Haithabu is the foundation of the author's argument that Göttrik's town of Sliethorp lay on this site, since the same ware occurs at Krinkberg in Schleswig-Holstein in association with Carolingian coins of the end of the eighth century; but his identification of the Kograben with the defensive wall which Göttrik is known to have built at present rests only on circumstantial evidence. In spite of a disturbed modern history, the main rampart of the Danewerke revealed in section as many as eight or nine separate building periods. The original rampart of earth, fronted by a wooden palisade and beyond that a ditch, was heightened several times. Later the woodwork was displaced by a wall with a rubble core and facing-stones laid in herringbone fashion, and later still the rubble wall gave way to a brick wall. The last stage can certainly be assigned to Waldemar at some date between 1160 and 1182, while the absence of stonework from the rampart of Haithabu itself suggests that the rubble wall belongs to a later period than the town, perhaps to the time of Knud Lavard. The author sees in the wood and earth rampart the reflection of a period such as occurs in the tenth century, when, with the establishment of a Saxon colony at Haithabu by Henry I and the creation of Schleswig-Holstein as the northern march of Germany, Haithabu itself lay beyond the frontier and in foreign hands. These chronological divisions, which contrast with those originally proposed by Sophus Müller, as well as the elucidation of various smaller lengths of rampart, can only be established by further digging. The excavation of some building directly associated with the main rampart would help towards this end; and although none such appears yet to have been found, it is difficult to think that a frontier of this length can have been completely without intermediate stations which could be used for signalling, if for nothing else.

Direct access to the sea, combined with a measure of security against attack from that direction, are conditions which governed the position and plan of a number of Viking settlements in the Baltic area. Nothing precisely similar is known from the British Isles, but Speed's map of Dublin (1610) which shows the walls enclosing a semicircular area of which the Liffey forms the diameter, suggests that this town may offer a western parallel to the more northerly sites noted by the author. In general the rampart of Haithabu resembled the earth and wood stage of the main rampart of the Danewerke, but the seventh period marked the introduction of a more elaborate fortification with a double parapet, the inner one being raised slightly above the outer. The author compares this type of rampart with the walls of Constantinople and, rather boldly perhaps, ascribes its introduction at Haithabu to direct oriental influence. Evidence recovered from within the town itself throws valuable light on many problems connected with Viking-age houses. The woodwork has decayed in the higher

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levels of the town, but in the lower levels the damp soil has preserved not only the foundations but also the lower stages of the house walls. One of the house types which seems to have been commonly in use in the town was an unpretentious, rectangular building, partly sunk below ground-level—a strong contrast with the hall or basilican type which is familiar from descriptions in the Icelandic sagas, as well as from excavation, and which is commonly regarded as the typical Viking house. The yield of small objects from Haithabu has so far been much less than from Birka, a circumstance which can be partly attributed to the stronger Christian element in the former. But in spite of this Dr Jankuhn has been able to draw a vivid picture of a thriving merchant town which Rimbert described in words remarkably similar to those used by Bede of London. In particular his survey of the pottery, in which he rejects the Frisian origin of the so-called 'Frisian' jugs favoured by Arbman and other northern archaeologists, demonstrates its close relations with the Rhineland.

There are times when the author allows himself to speculate rather freely. His remarks on Haddeby church are to the point, but it is questionable whether any importance can be attached to the existence of one of the so-called *Schwertsleifsteine* in the walls of the church. Furthermore one cannot refrain from criticizing his comments on the runic inscription from the foundations of Schleswig cathedral, apparently the earliest monument so far recovered from the site of the modern Schleswig. The stone is ornamented in an 11th century style and was erected to the memory of Halfdan, Sulki's son, who died in England and was buried at Skia. Even if the suggested identification of Skia with Skidby in Yorkshire is correct, it scarcely follows that Halfdan was probably amongst those killed at the battle of Stamford Bridge in 1066. These criticisms, however, do nothing to lessen the value of Dr Jankuhn's contribution to Viking-age studies and it remains only to hope that further excavation will enable him to solve the many problems which he has himself propounded.

PETER HUNTER BLAIR.

LE TERREMARE delle provincie di Modena, Reggio Emilia, Parma, Piacenza.
By GÖSTA SÄFLUND. Being no. 7 of the Acta of the Swedish Institute in Rome (agents, Gleerup, Lund or Harrassowitz, Leipzig). 1939. pp. 265 and 98 plates.

Those who have read my short article on this subject in *ANTIQUITY* for September 1939 will realize that the old theory of the Terremare as propounded by Pigorini and his school is completely dead. It is not so much for his further demonstration of this fact that we are grateful to Gösta Säflund, as for the facilities he now offers us for rebuilding a theory on a new and sounder basis. His volume consists of a critical digest of all the information to be derived from the

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original publications, together with a well co-ordinated series of illustrations from the Italian museums. It is a complete corpus of material for the four provinces in question. With its careful tabulation and complete system of references it is everything that the student of this subject could desire.

Säflund, very wisely, writes in Italian, which enables him to incorporate numerous extracts from the Italian writers without marring the literary quality of his own composition. It may be remarked that he can write with equal success in German (*cf.* his articles in *Studi Etruschi*), and that his selected bibliography includes all the relevant works in English and French.

Only by great industry as well as ability could so satisfactory a survey have been produced. The original records are somewhat scattered and are frequently very deficient in precision and detail. Indeed it was only the resuscitation of Coppi's forgotten volumes on the terramara of Gorzano (Modena, 1871-6) which provided a reliable standard for judgments of stratification.

A terramara is defined by Säflund as 'a station containing the typical bronze-age culture of central and western Emilia'. He expressly rejects the additions which Pigorini made, *viz.*, that such stations are ritually orientated, erected on piles and laid out on the system of castrametation found later in Roman camps. That Pigorini adopted this view must be attributed to his excessive preoccupation with one or two sites like Castione, which are now seen to be exceptional and not typical. In the majority of *terremare* there is no evidence of pile-construction, and it is likely that in some cases the supposed piles of an imitation lake-dwelling were merely the uprights of wattle huts. For essentially a *terremare* is no more and no less than a village of huts. It is frequently erected on an elevated site where any waterfilled moat would be impossible and a rampart is often unnecessary; only the peculiarities of a low-lying swampy station may render these additional features desirable.

Chronologically the sites here discussed range from the late Neolithic to the Early Iron Age. The flourishing time of the *Terremare* is in the Bronze Age, but not a few examples came down to the Iron Age, even to the Certosa period. There is no general or sudden abandonment of stations, but there is a gradual decline in prosperity as the Iron Age begins. Dating is established by the double evidence of typology and of stratification. The well-indexed plates make it possible to check and test every statement. Altogether a book to be warmly welcomed.

DAVID RANDALL-MACIVER.

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